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THE PENNSYLVANIA RAILROAD STOCK YARDS AND ABATTOIR.

UNTIL very recently the slaughtering of cattle for the Philadelphia market has been conducted exclusively in private slaughter-honses which are distributed all over the city. The same objections to such a system as exist in other cities, were also found in Philadelphia. The driving the cattle through the narrow and crowded streets, stabling them in inconvenient and unwholesome localities, the difficulty of promptly removing refuse and putrescent matter, the defiling the drains by blood, and choking them with solid obstructions, were amongst some of the many objections. In 1873 the Board of Health pointed out, in their annual report, the urgent necessity for establishing commercial abattoirs, and of abolishing gradually the numerous badly managed private slaughter-houses. Subsequently a scheme was projected and carried into effect by the Pennsylvania Railroad Company for the purpose of accomplishing this desirable object, and the result has been in all respects satisfactory, reflecting the utmost credit on the railway company. In 1875, after considerable opposition arising solely from objection to the site selected, the work of preparing the ground for the present abattoir was commenced and pushed forward with energy. In the early part of 1876 the buildings were sufficiently completed to be occupied, and the work of storing and slaughtering cattle has been in successful operation ever since, and Philadelphia possesses one of the most complete and commodious abattoirs in the world. It is situated on the west side of the Schuylkill river, a few hundred feet north of Market street, and is bounded on the north by Vine street, on the south by Arch street, on the east by the river, and on the west by Thirtieth street. It occupies an area of 21 acres. Cattle are discharged at the rear or western side of the abattoir, being brought thither on a branch from the main line, while towards the east another line runs, devoted entirely to sheep and hogs, and serving the portion set apart for these animals. These lines are lowered to the level of the pens. The enclosure is laid out in blocks and streets, which latter are all carefully paved and drained, and well lighted with gas. The accommodation it contains is as follows:

1. Cattle pens to hold 7,000 beasts.
2. Sheep pens, accommodating 10,000 animals.
3. Hog pens of about the same capacity.
4. Covered sheds for 500 cows and calves.
5. The main office and exchange building.
6. Stables for storing and selling horses.
7. The abattoir.
8. Fat and refuse reducing department.

The cattle pens are framed structures, only partially roofed in, so as to give the animals plenty of ventilation, and at the same time to protect them against sunshine and rain. The floors are all paved with granite, and well drained, so that no water or liquid impurities remains on the ground. All the pens, to the number of 172, are provided with water and food troughs, and it is a part of the regular business of the place for the attendants, after receiving notice of the approaching arrival of a number of cattle by train, to have the pens set apart for them placed in perfect order by the time they arrive, and to see that abundance of food and water is placed in the pen before they are turned into it, in order that the animals may be made comfortable after a more or less prolonged railway journey.

The sheep enclosures are two in number, each 350 ft. by 130 ft., and they are placed at the northern end of the establishment. These enclosures are roofed over and paved with granite blocks. Each building is divided up into suitable compartments and pens for wholesale or retail storage, fodder racks and water troughs being provided. Weighing apparatus are also furnished in each enclosure. The hog pens are situated to the east of the sheep depot.

Every provision has been made here to insure cleanliness and render the renewal of any portion of the structure easy, and considerable attention has been given to ventilating the enclosure. The roof of the building is supported on light iron columns, and the pens, etc., are divided off with iron railings. The floors of the pens are laid with Belgian blocks grouted in cement, and they are formed with sides sloping to a central gutter, falling in the other direction to a transverse drain. A stream of water flows constantly along the gutter, and carries off all impurities. Two rows of wrought-iron columns carry the roof, and divide the building into a central aisle 50 feet wide, and two side aisles 30 feet wide each. The middle portion of the building is covered with an arched roof springing 40 feet above the ground. The side aisles are 20 feet in height to springing, and have also a central roof. Along the sides of the building are a number of doors 6 feet wide and placed 15 feet apart, which are used to admit wagons for the purpose of loading the meat. Ventilators are placed over each door. Large windows are placed at each end of the central aisle, and further ventilation is secured by louvres running along the roof. The floor of the building is supported on cast-iron columns, and the space below forms a basement 11 feet in depth. Both the main floor and the basement are covered with an asphalte pavement 4 inches thick, having sufficient slope to insure perfect drainage. The work of slaughtering the cattle is carried on entirely on the main floor, that portion set aside for this purpose being divided off into pens, the floor of which is laid with heavy yellow pine planking carefully caulked. The cattle are admitted through doors in the ends of the building, through which they pass into the middle aisle, and thence through gates into the slaughtering pens, the centre space being fenced off from the sides by iron pipe railings. Each slaughtering pen is provided with the requisite apparatus, and with appliances for hanging up the carcases and dressed meat. The blood and refuse are removed to that part of the building set aside for their utilization, and an ample supply of hot and cold water is provided. In the winter the building is warmed by coils of steam pipes running around the walls. The place has a capacity for killing and dressing 1,200 beasts a day. The sheep are slaughtered in the basement of the west end of the building. Here there is a row of raised pens enclosed by a wire fence and iron posts, and paved with stone. In front of these pens is a stone table with a gutter running around it for catching the blood of the slaughtered animals. Three thousand sheep can be slaughtered and dressed in this department. In the basement at the east end of the building facing the river (the basement here is on the ground level) is the engine and boiler house, the former being 60 horse power and the latter equal to 100 horse power. Here also is placed the plant for reducing the tallow and treating the blood and refuse from the animals.

The latter department possesses considerable interest, and it contains several very novel features. It comprises two horizontal vessels or boilers 5 feet in diameter and 15 feet long, supported on cast-iron pillars. In each of these boilers there are three manholes 16 inches in diameter, two placed in the upper and one in the lower part. They are elevated so far above the basement that the upper opening is on a level with the slaughtering floor. The boilers are jacketed by being enclosed in a steam space 3 inches wide, and a shaft running in stuffing-boxes passes through the ends of the boiler. On this shaft are mounted radial arms, and on the end is a large spur wheel, gearing into a pinion which is driven from the engine by belting.

The rough fat resulting from each day's slaughtering is brought in trucks and filled into one of the boilers. The space within the jacket is then filled with steam and kept at a high temperature for four hours, at the end of which time the fat is entirely separated from the tissues and impurities. Water is then introduced into the bottom of the vessel, and the melted tallow is lifted and passed through a dome in the top of the boiler, and then is conveyed through a pipe for a distance of about 50 feet to the centre of the basement, when it is allowed to cool and settle, and is afterwards run off into casks. A length of glass tube is introduced into the pipe by which the tallow is taken off, so that the condition of the tallow can always be ascertained by the person in charge of the operation. As soon as all the fat is removed the water is drawn from the boiler and passes away into the river. When this has been done, the refuse from the slaughtering floor is placed in the tank, and the same process is repeated, the resulting fat being taken off, and the water running into the river. The day's produce of blood is then placed in the vessel, together with what refuse has remained from the two preceding operations, the tank is closed and again heated by the steam jacket, while the revolving arms are put into motion. All steam generated within the boiler escapes through a pipe in its lower part and is condensed in the river. After the drying and amalgamating action has been continued for a sufficient length of time, the blood and other matters are dried and brought into a powdered state, in which condition it leaves the vessel in a perfectly inoffensive condition. This apparatus was designed by Dr. J. J. Craven, of Jersey City.

The abattoir and buildings were designed by and erected under the superintendence of Messrs. Wilson Brothers, of Philadelphia, to whom considerable credit is due for having elaborated an exceedingly tasteful and appropriate structure. The internal arrangements were completed under the supervision of the Philadelphia Stock Yard Company, who have succeeded in supplying the building with all useful and efficient appliances, and have made it in all respects a model abattoir. We may add that large quantities of cattle are slaughtered there weekly, and shipped direct from Philadelphia to Liverpool.—*Engineering.*

ABATTOIR AND STOCK YARDS, PENNSYLVANIA RAILWAY, AT PHILADELPHIA.

ONE of the glass blower's secrets is, to simply moisten the mouth with a little water before blowing, when the water will be converted, in the interior of the drop, into steam, which greatly aids the breath in expanding the "bell."

DESIGN FOR A TRUSS BRIDGE.

By JNO. H. SNYDER, Richmond, Virginia.

THE inventor claims that it is cheaper in construction than any other railroad bridge of equal span. The horizontal tubes, from pier to pier, are made of boiler plate; the corners of angle iron, to which the boiler plate is riveted. The arch is constructed in the same way. The arch and horizontal tubes are united by rolled iron bars, dropping over the top of the tube, forming the arch and riveted to it, and to the horizontal tube below.

He alleges a great vertical strength of the horizontal tubes, on which the track is laid, and which is the tension rod for the support of the arch—the strength being increased in proportion to the weight bearing upon the arch. He affirms that a span can be made with perfect safety 600 feet long.

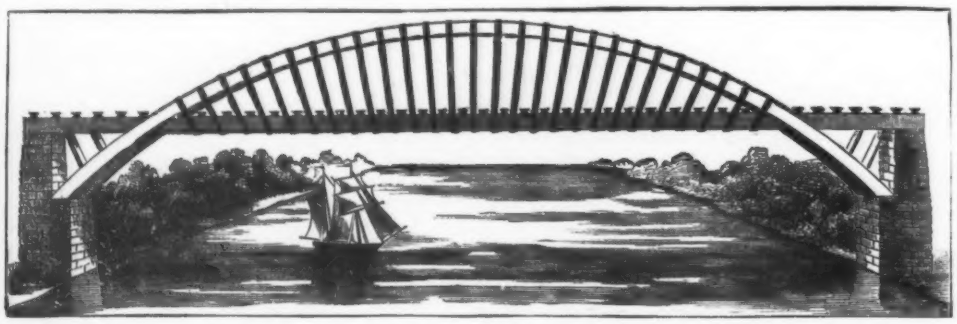
CONTINUOUS GIRDERS.

THAT the advantage of continuity in beams forced itself upon the attention of workers in wood and iron long before the question was discussed in its theoretical bearings, is evidenced enough by the primitive structures of our carpenters and ship builders. No pains were spared by them to secure continuity in rafters, purlines, joists, deck beams, keelsons, and, indeed, wherever strength or stiffness was essential. Modern authorities not unfrequently maintain, on theoretical grounds, that the continuity is of little or no advantage unless the span be considerable. We think this conclusion is not borne out by practice; the smallest span bridge to be found on a railway is the rail itself with its sleepers, and in no instance have greater efforts been made to secure perfect continuity by the adoption of deep fish plates and other expedients. In warehouse floors again, a 20 lb. rolled joist will do the work of one of 30 lb., if care be taken to secure "continuity" by fishing the joists together, not immediately over the columns, but at a point one-seventh the span distant from the same. The "bracket-plate" system of framing adopted in our ironclads is essentially an arrangement of continuous girders, and further, Lloyd's rules strictly enforce continuity and encastrement throughout the entire ship. Instances might be multiplied to show that the advantage of continuity may be most important at the smallest spans, no matter whether a live or a dead load be in question, whilst on the other hand it may be of economic value in very considerable spans as Mr. C. Bender has shown in an able paper read before the Society of American Engineers. It is necessary, therefore, that both engineers and architects should familiarise themselves with the principles governing the design of continuous beams.

There is no doubt whatever that in many instances independent girders have been adopted simply because the designer felt unable or unwilling to undertake the necessarily intricate and tedious preliminary work of calculating the strains upon the girders if continuous. We have always been of opinion that the instinctive distaste for these involved investigations is well founded, since we know that the deduced results after all, from falseness of hypotheses, must be merely approximative, and we believe the complexity of the investigation to be due solely to the subject being approached in the wrong way, and that if it were not so, a lad of ordinary intelligence should with half an hour's instruction be qualified to furnish the draughtsman with data at least as accurate as any to be found in the works of M. Bresse or other eminent authority on continuous beams.

Many of the simplest and most ordinary operations in an engineer's office, though capable of being effected by calculation alone, would, if so dealt with, present infinitely greater difficulties than any involved in the calculation of strains on continuous girders. Imagine, for instance, that the survey and level books for some line were placed in the hands of a mathematician with the request that he should calculate the minimum quantity of excavation required under certain limitations as to curves and gradients. He would reasonably reply that if practicable the task would involve more labor than a reduction of the transit of Venus. Submit the same problem to an engineer, and it will present to him no semblance of complexity. With the aid of straight edge, set square, protractor, and scales, he will plot his surveys and sections, lay on the center line and gradients, scale the depth of cutting, and having advanced so far by the aid of mechanical appliances, he will call in the aid of mathematics and complete his work by a proper application of the prismoidal formula. The mathematician may claim that since no straight edge is absolutely true, and no scale readable to a dozen places of decimals, results so derived must be inaccurate and of no scientific value. The engineer will retort that by his method he can attain any required degree of accuracy, and that in this respect, indeed, he is far better off than his friend the mathematician, who to prevent his calculations being literally interminable, would very probably have to assume that the ground is of one uniform gradient between certain points, when in reality it is undulating—which is in effect what the mathematician does when he assumes his continuous girder to be of uniform section throughout, although the very purpose of his calculation is to determine the required varying section at different points. If the mathematician takes account of the latter condition the calculations become quite unmanageable; Mr. Heppel once said that he had to work hard for six weeks to deduce the exact strains in the relatively very simple case of the Britannia Bridge. With the help of the equivalents of the straight edge, set square, and protractor, the strains might easily be obtained in as many hours.

In a continuous beam the load may be conceived as divided into two portions, one of which is carried by the beam as an independent girder and the other by the beam as a cantilever. If we know the proportions in which the load is divided the calculations, of course, become as simple as for an ordinary girder. All that is required to inform us of this is the weight or pressure with which the continuous beam presses upon the several piers and abutments, and having this the strains at any point can be obtained almost by inspection. The usual way of estimating the said pressure is by integrating the equation of the elastic curve, upon the assumption that the extension or compression of the extreme fibres at any cross section is inversely proportional to the total strain at that point, or in other words that the material is isotropic and the girder of uniform cross section. This is obviously as roundabout a way of arriving at the desired information as attempting to calculate the earthwork quantities on a given railway without first plotting the survey and section. We require to know the pressure with which an elastic beam bears upon certain supports, and we think if we were to put the case fairly to an untutored savage he would suggest that we should get an elastic beam and try. The suggested mode of procedure in a scientific point of view is on all fours with the straight edge and set square systems already universally accepted by engineers; the only questions that can arise are, is it as easy of application in practice as it appears to be, and are the results obtained entirely trustworthy and sufficiently exact for purposes of calculation? We can answer both of these questions in the affirmative; a strip of wood, some sheet lead to cut up into weights, and an ordinary letter-weighing scales are all the apparatus required, and so far as exactness is concerned, we have found the above apparatus so sensitive that it will indicate at once the relative difference in strain due to an engine traversing a continuous girder alone, and the same engine with the additional weight of the engine driver and stoker upon its foot plate; in short the exactness obtained is far in excess of that required or of that attained by any formula which we have hitherto had the good fortune to encounter. M. Bresse, who has advanced* no less than 1,200 formulæ for the simplification of the calculation of strains on continuous girders, confesses that he is but ill-satisfied with the results of his labors, since in accordance with the universal custom his formulæ are all based upon the false hypothesis of uniform section. The consequent inaccuracy in the result is considered by M. Bresse to be an unfortunate but unavoidable consequence of the intricacy of the problem. Our own experience leads us to a directly contrary conclusion; we have never found the least difficulty in introducing the very important element of the varying section of the girder in calculations of strains on continuous beams.



SNYDER'S DESIGN FOR A TRUSS BRIDGE.

We claim no novelty for the plan of making a strip of wood perform the work of integration; it has been in use by ourselves for the past fifteen years, and long antecedent to that Belidor, Parent, Barlow, and others investigated the question in a somewhat similar spirit. That the method has received so little attention from writers is probably due to their not having tested the system themselves, or they must have been struck with its simplicity and exactness. Not one, so far as we remember, has ventured to claim, as we do, that the strip of wood method properly applied is from a scientific point of view far more satisfactory than any purely mathematical method hitherto advanced, and that it offers indeed the only practicable means of arriving at the probable strains upon a continuous girder, as ordinarily constructed, to the degree of accuracy reasonably required, since by successively using several strips of wood of different quality a fair notion will be formed of the influence on the strains of the inevitably defective elasticity of the finished girder. The 1,200 formulæ of M. Bresse at first glance would appear to give results accurate to six figures, but a very cursory examination of the way in which they have been deduced proves but too clearly that from falseness of hypothesis, even the first of the six figures cannot always be relied upon, and that in very ordinary practical cases the error may amount to 10 per cent or more, apart from defective elasticity. The mode of procedure we have adopted in determining the strains on continuous girders of varying section is as follows: A strip of well seasoned, straight grained yellow pine 1·5 in. wide by 0·3 in. thick, and of sufficient length to cover the whole of the openings spanned by the continuous girder, is prepared to such a scale that the greatest span shall be about 40 in. in length. A suitable maximum distributed load for such a beam is 2 ounces per lineal inch, or 5 lb. on the 40 in. span. Small blocks of wood pinned down to the desk at the proper intervals represent the piers, and upon them the beam is laid flatwise. A loop of thread passed round the beam is attached to one arm of a letter scales, and the pressure of an unloaded beam upon each point of support is successively measured. The sum of these will of course be equal to the total weight of the strip of wood, but the distribution of the same upon the several piers will be found to follow no law. This simply indicates that the strip is not normally straight, and is of no consequence, as the actual pressures should be recorded and deducted from the measured pressures of the loaded beam, by which means the weight of the beam itself will be eliminated. Assume that our strip traverses three spans of 40 in., and that its weight in all is 15 ounces, or 5 ounces per span; then theory indicates that the pressure upon the end supports, if the beam is normally straight, will be 2 ounces, and upon the central supports 5½ ounces, whereas by experiment the pressures may prove to be 5½ ounces on the end and 2 ounces on the middle, although, it is true, if there were any such vibration the strip would be visibly crooked. If thought desirable the beam can be reversed and weighed in that position with and without the load, but we have seldom found this necessary, as so many checks arise at different stages of the process that any appreciable defect of elasticity is at once detected, and it is time enough then to duplicate the experiments.

The next step is to distribute upon the upper surface of the beam weights (conveniently made of strips of sheet lead folded up) and to measure the resultant pressure upon each of the supports by means of the loop of thread and letter scales as before. This will be repeated under all the required variations in the distribution of the load, remembering that the maximum strain over any pier occurs when the two contiguous spans alone are loaded, and the maximum at or near the center of any span when that span alone is loaded. A first check upon any of these measurements is obtained by taking the sum of the pressures, which must of course be equal to the total weight of the beam and its load, however distributed; it will be found that there is no difficulty whatever in weighing and balancing the pressures to the eighth of an ounce.

Having thus obtained the pressures upon the points of support of our continuous beam of uniform section, we may complete the calculations of strains as readily as if we were dealing with a succession of independent spans. We first obtain, by a process of subtraction, the shearing stresses at the ends of each bay of the continuous girder, or, in other words, divide the total pressure upon each pier into the two pressures due to the girders on either side of it. Reverting to our example of three spans of 40 in., and assuming the distributed load to be 2 oz. per lineal inch, or 15 lb. on the entire girder, we find the pressure on the end piers to be 32 oz., and on the central piers, 88 oz. Since the weight of each bay of 40 in. is 80 oz., we see at a glance that the 88 oz. on the central pier is made up of $80-32=48$ oz. from the end span, and $88-48=40$ oz. from the center span. Similarly for any other distribution of the load having the total pressure upon the piers we can, by a process of subtraction, arrive at the shearing stresses. But these shearing stresses inform us also at a glance how much of the load is carried by the girder, as a simple beam and how much as a cantilever. Thus, if the girder had not been continuous the pressure upon the end support must obviously have been $\frac{80}{2}=40$ oz., whereas it is 32 oz. The difference of 8 oz. is found in the 88 oz. on the central pier, and it could only have been transmitted there by the girder acting as a cantilever, hence the consequent moment at the central piers must necessarily be 8 oz. $\times$ 40 in. $=320$ inch-ounces. If we have our curve of moments for the independent span plotted it is only necessary to set up this moment to the same scale at the central pier and connect it by a straight line with the opposite extremity of the diagram, when we obtain at once a complete diagram of the strains upon the end span of our continuous girder. The maximum moment for the independent span would of course be 80 oz. $\times 40 \div 8 = 400$, hence the maximum strain upon our girder of 3 equal spans of uniform action, and uniformly loaded, will be 20 per cent. less than on the independent girder.

The moment thus found for the first pier will occur at every succeeding pier, no matter how long the continuous girder may be, unless something arises to modify it, and we can tell at a glance whether anything does arise by noting if the shearing stresses at the ends of any bay are different from what they would have been had the girder been independent. If there is a difference the moment will be correspondingly changed plus or minus. Thus the shearing stress at the ends of the center bay of our three-span continuous girder is 40 ounces or the same as if the girder were independent, and we know, therefore, that the moment of 320 inch-ounces will recur unchanged over the second pier. Had the pressure been 40 ounces at one end and 30 ounces at the other with an uniformly distributed load, it is obvious that the moment would be changed, since, if independent, the shearing stress would have been $\frac{40+30}{2}=35$ ounces at each end. It is equally obvious that the $35-30=5$ ounces, from which the second pier has been relieved, must have been transmitted to the first pier by the girder as a cantilever, and the moment 5 ounces $\times$ 40 in. $=200$ inch-ounces so developed will dispose of the original moment of 320 ounces to that extent, and prevent more than the remaining 120 ounces being passed on to the second pier. Similarly, no matter how the load may be distributed we can arrive almost by inspection at the moment over each pier, and then by connecting these points on our diagram by straight lines, we at once change the diagram for independent spans into the corresponding diagram for the continuous girder.

Our diagram for the continuous girder of *uniform section* being completed, we have advanced the matter as far as is customary in ordinary methods of investigation, but by no means as far as is desirable or even necessary. We proceed to reduce the width of our 1·5 in. strip of wood guided by our first diagrams, in some such proportion as we should reduce the sectional area of the flanges in the actual girder. We then load the strip as before and repeat the weighings, which in general will be found considerably modified, and this modification will, of course, appear in the diagrams and effect the estimate of strains materially. The few strokes of the chisel required to shape the wood thus effect what such able and earnest men as M. Bresse have reluctantly given up as hopeless to attempt to attain.

By the aid of the strip of wood and letter scales we are enabled to substitute a simple and even fascinating experiment for weeks of wearying and uninteresting trial calculations. So far as accuracy is concerned, the strip of wood is far more perfectly elastic than any riveted girder, and we may, if great refinement be required, take the mean of the results obtained with two or three different strips of wood or even with the same strips reversed, and as we have before said, we can readily weigh to one-eighth of an ounce, which is equivalent to about 8 cwt. in the instance of a continuous girder for a single line railway bridge of 100 ft. span. So far as the propriety of the method from a scientific or rather sentimental point of view is concerned, we are inclined to think that an engineer is as free to avail himself of the aid of a strip of wood in determining the strains upon a continuous girder of varying section, as ordinarily constructed, as he is to refer to his watch instead of a Nautical Almanac and astronomical observations when he wishes to know the hour, for neither theoretically nor practically is the watch a more perfect appliance for its purpose than is our straight grained, well seasoned, strip of wood.—*Engineering.*

* "Cours de Mécanique Appliquée."

"POTENTIAL" ENERGY.

By S. TOLVER PRESTON.

1. THE tendency of scientific progress is to look to the existence of physical processes in all phenomena, and consequently theories which are intended to supplant such processes do not meet with much encouragement. This is true of the theory of "action at a distance," now abandoned by some of the foremost minds. When a stone is thrown vertically into the air, we are led to look to some physical process, whereby the stone is brought to rest. If the theory of "action at a distance" (*i.e.*, the theory of an action upon the stone without the intervention of matter) be abandoned, then it inevitably follows that the stone was brought to rest by the transference of its motion to external matter, for its motion cannot have been annihilated or ceased to exist spontaneously. Two assumptions are alone possible; either the energy of the stone, represented by its motion, was expended in overcoming "attraction" (as used in the sense of a supposed pulling power without the intervention of matter): or this energy was expended in communicating motion to external matter (in some invisible form). The communication of motion is the communication of *kinetic* energy. Hence, if the idea of "attraction" (as used in the sense of an action at a distance independent of matter) be abandoned; then the energy of the stone must have been employed in generating *kinetic* energy. It is therefore not logical to assume (by the abandonment of the theory of action at a distance) that such an energy as "potential" energy exists. Professors Tait and Balfour Stewart in their recent work ("The Unseen Universe," page 109), say: "Of course the assumption of 'action at a distance' may be made to account for anything; but it is impossible (as Newton long ago pointed out in his celebrated letters to Bentley) for any one who has in philosophical matters a competent faculty of thinking for a moment to admit the possibility of such action." Again at page 110: "If Le Sage's theory or anything of a similar nature be at all a representation of the mechanism of gravitation, a fatal blow is dealt to the idea of a tranquil form of power we have called 'potential' energy. Not that there will cease to be a profound difference between it and ordinary kinetic energy; but that both will henceforth require to be regarded as *kinetic*."

2. If, therefore the theory of "action at a distance" be thus widely disclaimed, there does seem to be some inconsistency in the extensive and common use of the term "potential" energy. This term involves the existence of two kinds of energy ("potential" and "kinetic"), that energy can have, as it were, a double nature, an assumption which surely in itself is sufficiently questionable.* We can realize clearly energy in the form of matter in motion; what conception, on the other hand, can we form of an energy without motion, and if not, what right, or rather what power, have we to put forward the assumption of its existence? Is it not rather more logical to assume that the apparent disappearance of motion in certain cases lies in our absence of means to analyze the case or ignorance of physical causation, than to assume that motion has been annihilated, or has assumed some mysterious form of which no clear conception is possible. It should be observed that a grand principle lies involved in this question. If we reject the theory of "potential" energy (as we are bound to do if we reject the theory of "action at a distance"), then it follows that motion is indestructible, for that motion should cease spontaneously, or be generated spontaneously, is a violation of the fundamental axiom that an effect cannot take place without a cause, or of the fundamental quality of the *inertia* of matter. Thus if we reject the assumption of "potential" energy, or the assumption of the existence of an energy which is not kinetic, then the great principle of the indestructibility of motion inevitably presents itself for acceptance. All physical phenomena come thus to be fundamentally correlated as phenomena of motion becomes the fundamental groundwork underlying all phenomena. This principle is grand from its simplicity. Simplicity is admitted to be the necessary accompaniment to order, and phenomena of great diversity are best derived from a simple basis or groundwork, just as the simplest possible mechanism is best adapted to produce precise and complex effects.

3. It is sometimes argued that because the position of a raised weight (at a distance from the earth) allows energy to be transferred to it, or the weight *can* fall, that therefore it may be said to have "potential" energy, and that independently of any theory as to where the energy is derived at its fall. This is clearly erroneous. For since by the rejection of the theory of "at a distance," the energy expended in raising the weight was necessarily employed in generating *kinetic* energy, it cannot have been employed in generating "potential" energy (*i.e.* an energy which is not kinetic) as well, for a double equivalent of energy cannot be generated. This appears to be rather a common error, and therefore it may be as well to point it out. It may be observed that the general conclusion that the work done in raising the weight consists in the communication of motion to matter, follows inevitably on the abandonment of the theory of "action at a distance," entirely independently of all question as to the *process* involved. The process involved or precise mode of action may properly form a subject for future investigation. The fact of the transference of the motion to matter is the legitimate and logical deduction from the principle of Conservation, and this principle would be of little use unless we could apply it to such a case as this; for evidently the necessary preliminary towards the investigation of the process of transference of the motion is the previous recognition of the fact of transference. The motion must have been either transferred or annihilated, and for motion to be annihilated, or to go out of existence spontaneously, is contrary to the principle of conservation and to the accepted axiom that an effect cannot take place without a cause.

4. It is evident that by the rejection of the theory of "action at a distance," every case of the approach or tendency to approach of masses and molecules of matter can in principle be solely referred to the impulsive action of external matter, or of the particles of a medium, producing an excess of pressure upon the remote sides of the molecules or masses, this holding true whether the tendency of two molecules to approach one another be due to a motion of the molecules themselves (producing a disturbance of the equilibrium of pressure of the medium between them), or to a motion of the medium itself in the form of streams or currents acting upon the molecules, as in accordance with the gravitation theory of Le Sage.

5. Let us suppose a disk suspended so as to move freely in a horizontal line, and which tends to approach another disk opposed to it, under the action of an invisible jet of air which impinges continually against the remote side of the movable disk. Then the one disk, tending to approach the other without any visible agent, would present the appearance of being "attracted" to the opposite disk. Let us suppose further that a sudden impulse is given to the movable disk so as to give it a certain initial velocity which carries it away from the opposite disk which it tends to approach. The disk which has received the impulse will gradually lose its motion, finally stop, and then recoil with freshly acquired energy towards the opposite disk. Supposing, therefore, the physical conditions to be here unknown, then the case precisely represents the appearances of a case to which the allied theories of "action at a distance" and "potential" energy are applied. When the disk recedes, it loses its motion by transferring that motion to the invisible molecules of the air jet. Conversely, when the disk recoils or returns by the same path, it acquires motion by transference of motion from the molecules of the air jet to it. If therefore (supposing the physical conditions to be unknown) we were to apply the same reasoning here, the inference would be that the motion lost by the receding disk was not necessarily transferred to matter, but that this motion took up a different form "(potential" energy so termed), and that this form which is *not* motion, was reconverted into motion at the recoil of the disk. If, now, in a case where the physical conditions are recognized, the above theory appear untenable and even absurd, then as a point of principle it can be none the less so in a case where the physical conditions are not recognized. An earnest protest ought to be entered at the present time against the theory as opposed to logical reasonings, and it ought not to be necessary to wait until at some future time the process is discovered before the untenable character of the theory is recognized. The use of our reasoning powers is to predict the existence of a physical process, the use of investigation is to discover its nature, and the recognition of its existence is the first step towards searching after its nature. It would surely be arrogant to assume that our powers of investigation are so complete as in every case to be able to trace a motion, and experimentally prove its existence, and therefore it would be arrogance to assume that the motion has ceased to exist or can have assumed a different form ("potential" energy), *i.e.*, has ceased to exist in kinetic energy, simply because we are unable experimentally to trace it.

6. Again, the assumption (before referred to) that one occasionally meets with, viz., that a raised weight has "potential" energy, because its position (at a distance from the earth) allows it to approach the earth, is exactly comparable to the assumption that the disk in the above illustrative case (after receding) has "potential" energy, because its position allows it to approach the opposite disk, or that a ship has "potential" energy when blown against a rock, because its previous position, at a distance from the rock, allows it to approach the rock. This would be evidently parallel reasoning, if it were legitimate to say that a body (such as a raised weight) had "potential" energy, simply because its position allowed motion to be transferred to it. The raised weight simply represents a case of equilibrium, which may be rendered unstable or stable at will.

7. We bring forward the above arguments against the theory of the existence of an energy which is *not* kinetic in the true interests of progress, for clearly the aim ought to be to encourage the investigation of natural processes, and to direct as much as possible the attention to tracing those motions which apparently disappear, not to put forward a theory which by assuming that the motions have ceased to exist (*i.e.* have ceased to be kinetic) tends to interfere with progress, and to prevent that clearness of conception which is the essential characteristic of truth.

There are signs not wanting that to regard energy as having but one character, and all physical phenomena as phenomena of motion, together with the acceptance of the great principle of the indestructibility of motion, is the goal towards which modern science is tending.—*Engineering.*

* To suppose that energy can assume two forms essentially different, or can possess a duplex character, cannot be otherwise regarded than as comparable to the assumption that any other of our fundamental conceptions, such as time or space can possess a duplex character. If this be inconceivable, then certainly the assumption of the existence of an energy *without motion*, cannot be said to have greater clearness to recommend it; for motion is the very attribute which we involuntarily attach to energy in order to have any conception of it at all. If the assumption of "potential" energy (*i.e.*, the assumption of the existence of an energy which is *not* kinetic) had never been put forward, then the distinctive prefix "kinetic" would never have been required. It would have been simply sufficient to have used the term "*energy*," for any prefix implies that energy can have a duplex character. It may be observed that Sir John Herschel in his well-known essay, "On the Origin of Force" remarks of potential energy in the terms—"a phrase which I cannot help regarding as unfortunate."

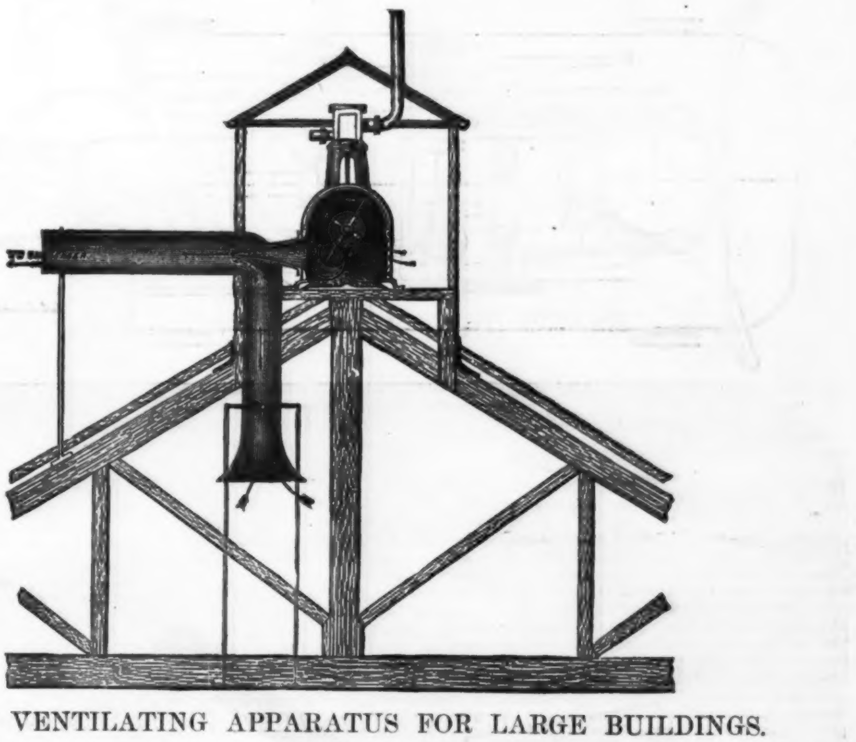
NATURAL GAS AT SACRAMENTO.

THE boring of an artesian well at Sacramento shows the character of the underlying strata. The *Record-Union* says:

After passing through strata of loose sand, gravel clay, boulders, etc., the tools at a depth of 886 feet struck soapstone, then passed into cemented sand, and at 1,090 feet struck wood, which was met with at intervals until a depth of 1,108 feet was reached. This wood is black, but in a good state of preservation, looks like pine or redwood, and remains solid after exposure to the air. Soapstone followed the cemented sand, and was in turn followed by cemented sand at a depth of 1,160 feet, and the boring tools have been in that substance ever since. The tubing is full of water to within about 50 feet of the surface of the ground. About the time a depth of 1,075 feet was attained gas commenced bubbling up through the water, and one night when a candle was lowered down to look at the water there was quite a little explosion. Last Sunday the hole in the tubing was covered over at the top, leaving an opening in the center through which a piece of half-inch gas pipe was passed. The gas escaping through this pipe was ignited and blazed up to a height of two feet and a half, burning with a bluish flame, but neither when burning nor otherwise did it emit any odor. Monday forenoon the gas would not ignite, but that afternoon it burned freely again. Wood has been found at several depths besides that mentioned above, the lowest being 1,320 feet, when some of the pieces taken out had a coating of cemented sand, showing that the stick of timber from which it was taken was lying in that species of deposit.

NEW VENTILATING APPARATUS.

THE apparatus we illustrate was exhibited at the recent Liverpool show of the Royal Agricultural Society, by the Saville Street Foundry Company, Sheffield. The apparatus has been invented by Mr. C. S. Hall. The object sought is to ventilate and pass noxious vapors, gases, fumes, etc., from the mixing rooms of chemical works, to the chimney, furnace, or condenser for decomposition. These gases are composed of elements destructive to cast iron, and though fans are largely used, yet as the gases must pass through them they are soon attacked and destroyed. Mr. Hall employs a Baker blower as shown, placed on the top of the roof of the room to be ventilated, and outside. With this a jet of air at a pressure of from 1¾ lb. to 2½ lb. per square inch is forced into a large tube which communicates with the chamber below, and so by induction takes the gases from below and sends them to the condenser, etc., without even passing near the blower. Thus Mr. Hall can effectually deal with the foulest and deadliest fumes; about six times the volume of the jet can be exhausted. This is a very neat adaptation of a special machine to a special purpose, and ought to prove very satisfactory in its operation.

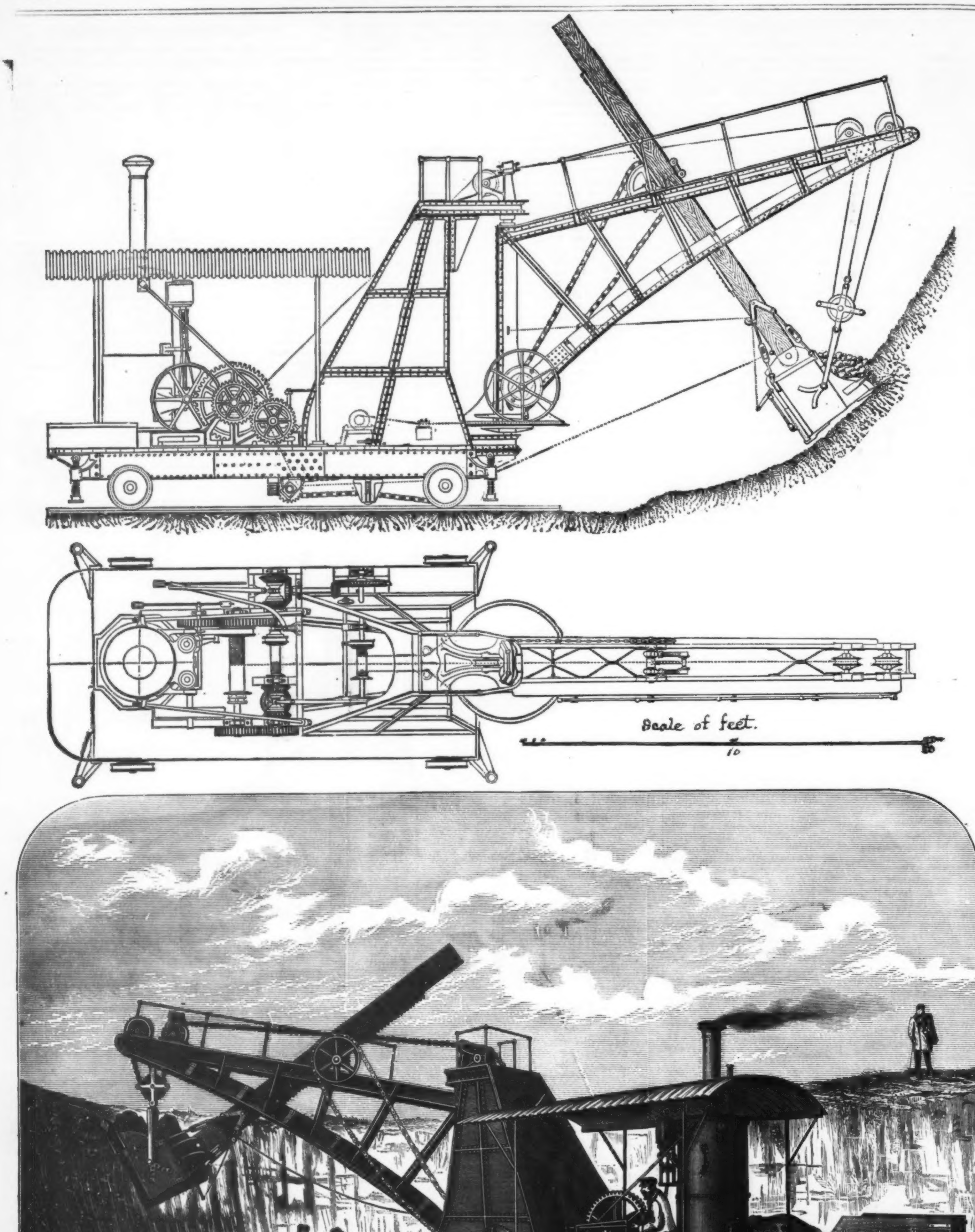


NEW VENTILATING APPARATUS FOR LARGE BUILDINGS.

FRICTION OF PITCH.

M. v. OBERMAYER, of Vienna, has proved by experiments that the internal friction (viscosity) of hard black pitch is subject to the same laws as fluid friction. He determined the co-efficients of internal friction by three different methods: (1) Pressure of cylindrical plates; (2) Deformation of parallelopipedal plates; (3) Distortion of cylindrical plates. No gliding of the black pitch occurs on the metal plates, between which the pitch plates are cast. For soft bodies, the internal friction, says *Nature*, appears not to follow exactly the laws of fluid friction.

NEW SALT OF IRON FOR STEELING COPPER PLATES FOR ENGRAVERS.—M. R. Bœttinger.—The author dissolves 10 grms. prussiate of potash and 20 grms. salt of seignette in 200 c.c. of water. He then adds 8 grms. of ferric sulphate in 50 c.c. of water, when a precipitate of Prussian blue is produced. A solution of caustic soda is then added drop by drop till this precipitate is re-dissolved. A clear yellow solution is thus obtained which may be used for depositing iron upon copper. The same liquid may serve to dye cloth blue. It is steeped in the liquid, dried in the air, passed into dilute sulphuric acid at 2 per cent., washed and dried.—*Chemisches Centralblatt.*



STEAM EXCAVATING MACHINE. BY DUNBAR & RUSTON.

STEAM EXCAVATING MACHINE.

THE Steam Excavator, of the general form here shown, is a mechanism of American origin; but is now used and manufactured in various parts of the world. We illustrate in section and perspective the machine designed by Messrs. Dunbar & Ruston, as made by Ruston, Proctor & Co., of Lincoln, Eng.

As the machine advances, excavating its own gullet, it fills alternately, first on the one side and then on the other, one of the empty wagons in position for being filled. The lines of rails are arranged for the wagons so that there is always a train of empty wagons standing on a central road behind the "navvy," and from whence they are drawn over a short jump road into position on the side roads for filling, while the filled wagons run back from the machine on the side roads. The "navvy" illustrated is capable of excavating and filling into wagons at the rate of 60 cubic yards per hour, two men and one boy being required to work it.

This machine, as will be seen on reference to the detailed drawings, is constructed mainly of wrought iron, so as to withstand the heavy work that it has to encounter. The mode of working it may be briefly described as follows: The engine-driver, who has the control of all the moving parts, is directed by the man who has charge of the scoop, and who stands on the circular platform at the foot of the jib in front of the machine. When the jib is swung to the position required, the scoop is lowered till the mouth of it rests upon the ground. The man on the circular platform by means of a foot-brake and gear holds the scoop in that position, so fixing the length of the scoop handle from a pivot or point on the jib. The scoop is now drawn forward by means of a chain and winding drum, thereby cutting all before it, according to the radius described by the length of the scoop handle. As soon as the scoop is filled, the man who has charge of it eases the foot-brake, allowing it to come out of its cut. When lifted high enough, the jib is then swung round until the scoop is brought over the wagon to be filled; the attendant now by means of a trigger line draws the spring catch bolt, allowing the hinged bottom to drop down, discharging its contents into the wagon. The jib is then swung round again, the scoop lowered, and the operation repeated.

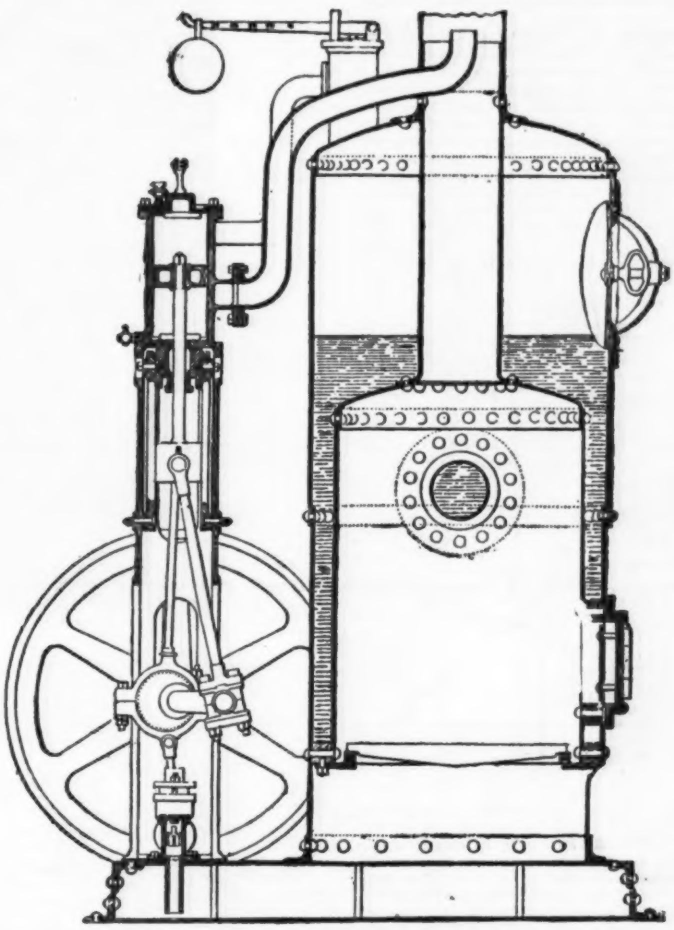
After the machine has excavated all that is within its reach, the anchor screws are slackened off, extra sleepers with a short length of rails are then laid down in front of it, and by means of the propelling gear it is moved forward the required distance. The anchor screws are then screwed down in order to prevent the machine from slipping back when at work.—*Engineering.*

STRAW AND COAL BURNING ENGINE.

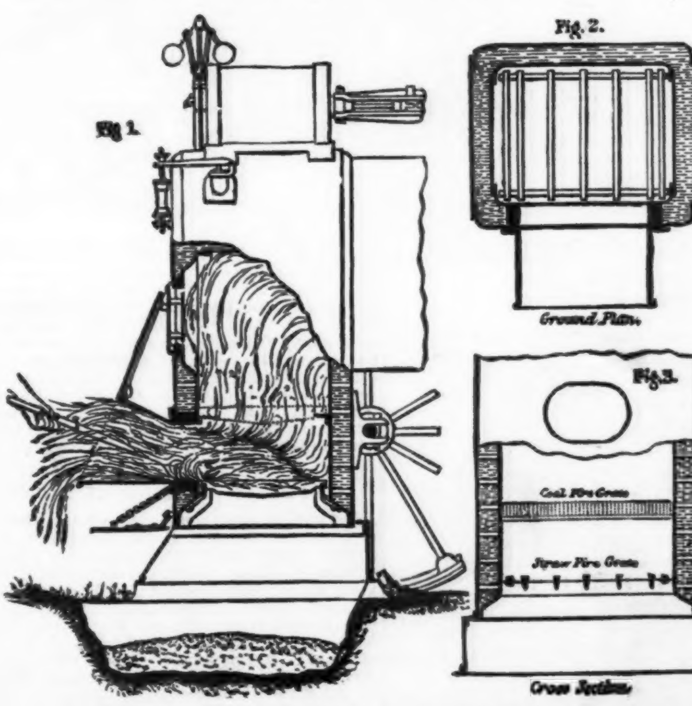
AT the recent show of the Royal Agricultural Society, Liverpool, Messrs. Garrett and Sons exhibited a 10 horse power straw-burning engine on Kotzo's system. Referring to the engraving it will be seen that the engine is provided with a somewhat deep firebox, there being a large opening in this firebox below the ordinary fire floor, and this opening being provided with a conical mouthpiece through which the straw is fed by hand. When the engine is worked with coal the grate is fixed above the level of this mouthpiece, as shown by the dotted lines. The straw firegrate is formed of bevelled wrought-iron bars 2 in. deep by $\frac{3}{4}$ in. thick at the top and $\frac{3}{8}$ in. at the bottom, these bars being placed about 4 in. apart at the center of the grate, while at the sides the ingress of air is checked by the introduction of a bar $\frac{7}{8}$ in. square placed about $1\frac{1}{2}$ in. from the firebox side and about an inch from the first bevelled bar. In one of their 8 horse power straw-burning engines Messrs. Garrett and Sons provide 4.2 square feet of grate, while the firebox surface is 26 square feet and the tube surface 132 square feet, making 158 square feet of heating surface in all. The ratio of grate to heating surface is thus 1 to 37.6. The tubes are $2\frac{1}{4}$ in. in diameter. Of course, with an arrangement of this kind much depends upon the skill of the stoker, but with proper care the engines constructed on M. Kotzo's system appear to burn the straw very efficiently, and in course of the discussion on Mr. John Head's paper "On the Combustion of Refuse Vegetable Substances," read before the Institution of Civil Engineers last January, Mr. Frank Garrett gave some particulars of the work done by an eight-horse engine of this type. These particulars we tabulate as follows:

Date of Trial.	Quantity of Wheat thrashed and dressed.	Quantity of Straw burned.	Quantity of Water evaporated.	Pounds of Water evaporated per pound of Straw.
	quarters.	lb.	lb.	lb.
May 28, 1875....	30	1582	2280	1·7
June 7 "	48	2196	3504	1·6
" 8 "	$25\frac{1}{4}$	1203	1776	1·4

During these three experiments there were thus 4981 lbs. of straw burnt and 8160 lbs. of water evaporated, the average evaporation per pound of straw being thus 1.6 lb. As the total quantity of wheat thrashed and dressed was $103\frac{1}{4}$ quarters, the quantity of straw burnt amounted to 48.1 lbs. a quarter. Mr. Garrett added that these trials were made during somewhat unfavorable weather, and that he anticipated much better evaporative results from straw in better condition.—*Engineering.*



NEW VERTICAL ENGINE AND BOILER.



STRAW AND COAL BURNING ENGINE.

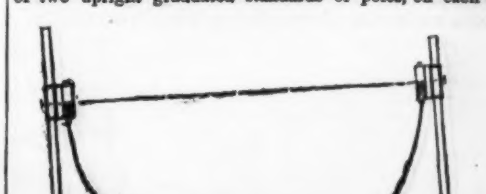
NEW VERTICAL ENGINE AND BOILER.

AMONG the exhibitors at the Royal Agricultural Society's Show this year at Liverpool were Messrs. Alexander Shanks and Son, of Arbroath, who had a collection of their new pattern of vertical engine and boilers. The engraving will explain the features of the engine, which is very neatly worked out in its details. The boiler is the ordinary vertical pattern, with conical cross tubes in the fire-box, the shell, however, being carried down below the fire-box ring so as to form the sides of the ash-pit. The boiler stands on a water-tank base plate, on which the feed becomes warmed and on which the engine also is mounted. The engine has a neat hollow cast-iron frame of the steam-hammer type, this frame being distinct from the boiler. The engine is controlled by a Buss governor acting on an equilibrium valve, and in the larger sizes an expansion valve, adjustable by hand, is also provided.

THE "AITA" LEVELING APPARATUS.

By HENRY ECROYD.

A NEW leveling apparatus has been designed by Signor Dottore Luigi Aita, C.E., of Padua, Italy. Dr. Aita has recently been appointed to superintend the construction of important sanitary works which will secure to the city of Padua an efficient drainage and water supply. Hitherto it has continued to draw the latter from its shallow "pozzos" or wells (as in the days of Palladius, Galileo, and Giotto), while the drainage has been allowed to find its own way into the canals which intersect the city, and which communicate with the river Brenta on the one hand and the river Baccaglione on the other. Dr. Aita, in attempting to take the preliminary levels, found it difficult, on account of the narrow and tortuous streets and the porticoed thoroughfares, to do the work expeditiously with the usual instruments. He therefore set to work to devise a better instrument, and succeeded in perfecting the new apparatus now described. It consists of two upright graduated standards or poles, on each of

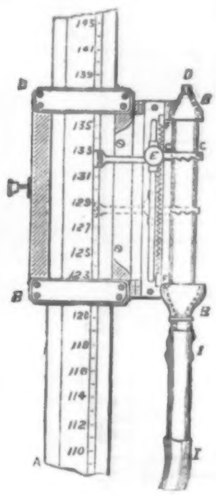


which slides a brass framework arrangement attached to a glass tube. These glass tubes, or cylinders, are connected together by an india-rubber pipe of any length required, which is filled with a colored liquid, the quantity being so regulated that it shall more than fill the pipe, and rise about half way up the glass tubes when they are placed parallel to each other, as seen in the accompanying sketch. The Aita level has already been in constant use for more than three months, and its inventor finds it admirably adapted for the requirements he had in view. The observations he has taken with it are much more accurate than any obtainable by the most careful use of the theodolite, while the saving in time is very great, Dr. Aita having been able to take 140 levels, covering 2 kilometers of ground, during a working day of six hours, most of them taken in positions in which it would have been impossible to take observations at all by the old system. With the new instrument levels can be taken round corners, and through winding passages, and by night, as well as by day. It is simple in construction, and is always in working order, and can be used by the most inexperienced operator, as the liquid in the tube finds its own level, and indicates on the graduated scale the most exact results desired.

Dr. Aita has declined to secure patent rights for his instrument, either in his own country or elsewhere, as he desires that his professional brethren everywhere may benefit by its use. For taking underground levels, as well as observations within cities and towns, and among trees and standing crops, it will be found invaluable. The eyes of the operator are not fatigued. The time required for the liquid to find its level in the tubes after the standards are placed in position is about five seconds.

The following drawing, with exceptions given below, will enable any of your readers to have an instrument constructed for themselves.

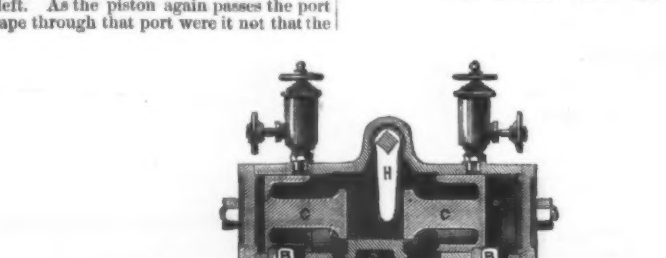
A A, upright standard or rod, in duplicate, made of well-seasoned lance-wood, about 8ft. high by 2½in. broad and 1in. thick. Down the center is let in a graduated scale, 1½in. wide, in brass or ivory, which is graduated in centimeters. B B, brass frame sliding upon the standards and fitted with glass tube and sliding scale. C, screw to fix the frame B B to A A. D, india-rubber stopper or plug to fix the top end of the glass tube when the instrument is not in use, and thus prevent the escape of the liquid—this plug must be withdrawn when an observation is taken—E, screw working the indicators H and G up and down the frame F F. F F, rack and pinion frame. G G, ring surrounding the tube, which, when lowered to the surface of the liquid in the tube, indicates, by means of the indicator H, the height of



the liquid on the graduated scale. H, pointer or indicator, graduated in half millimeters. I I, india-rubber tube, ⅜in. diameter, cased in canvas. This tube may be of any length the operator may find most convenient. Dr. Aita uses 30 meters long, filled to about the midde of each glass tube with a liquid composed of one part of claret wine mixed with five parts of water. This is carried on the shoulder of an assistant in a coil, 2ft. diameter, and is uncoiled as the operator passes along from one point of observation to another. The writer would suggest as an improvement upon this plan that the tube should be lined with spiral wire, so as to prevent it from doubling or knotting when being paid out, and thus secure a free flow of the liquid. He would also suggest that a rotary frame or drum, mounted on wheels, similar to those used for garden hose, should be used, on which to coil the tube, instead of having it carried by the assistant on his shoulder.—*The Engineer.*

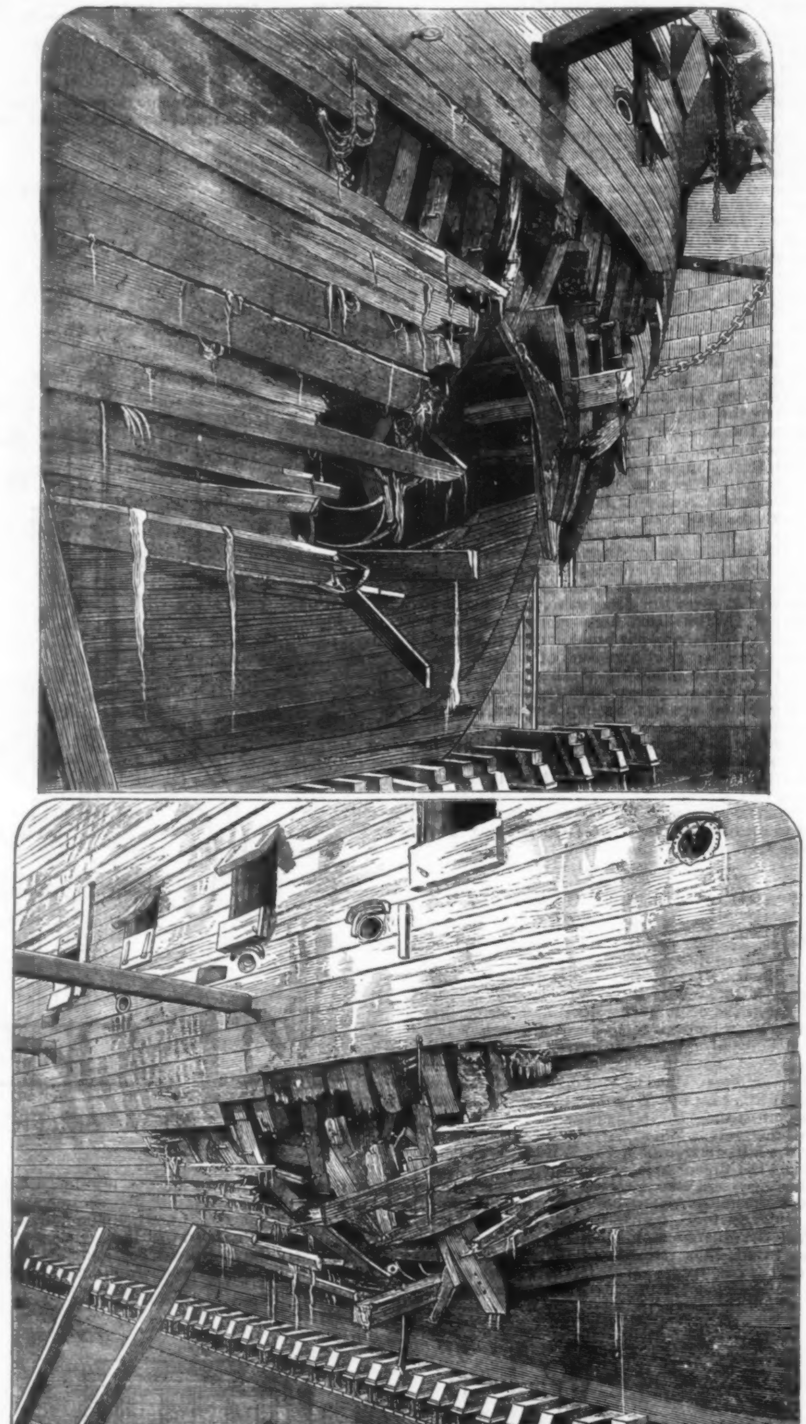
IMPROVED STEAM PUMP.

THE Imperial Pump and Engineering Company, of Smethwick, England, exhibited at the recent show of the Royal Agricultural Society, Liverpool, a good collection of their pumps. We give a section which will explain the action of the horizontal pumps made by this firm. Supposing the piston to have just completed its stroke towards the right, and to be in the position shown in our illustration, it will have just passed the small port D, and the steam which was behind it will thus pass up through this port, and lifting the auxiliary valve A, will flow through the small port E to the space at the right-hand end of the plunger C, thus forcing it over into the position in which it is shown in our engraving, carrying with it the main slide valve. The steam then flows through the passage K to the right-hand end of the main cylinder, and the piston commences its stroke towards the left. As the piston again passes the port D, steam would escape through that port were it not that the



IMPROVED STEAM PUMP.

movement of the plunger C to the left, in addition to shifting the main slide, also admits steam through a small port F to the top of the auxiliary valve A, and replaces it on its seat, thus shutting off the communication with the valve cylinder. During the stroke in the opposite direction, the action above described is repeated at the opposite ends of the cylinder. As will be seen from this description, the arrangement is a very simple one, and the pumps shown at Liverpool work very steadily.—*Engineering.*



EFFECTS OF TORPEDOES ON THE HULLS OF VESSELS.

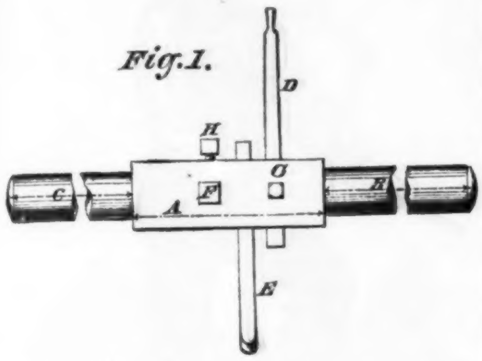
FRENCH TORPEDO EXPERIMENTS.

WE publish engravings, showing the effect of a torpedo explosion directed against the Bayonnaise, an old wooden French frigate of about 2,000 tons. The experiments, which terminated in the practical destruction of the vessel, took place during February and March last off Cherbourg. On the 3d of March a torpedo charged with 33 lb. of damp guncotton, and carried at the end of a steel spar about 40 ft. in length, was exploded against the side of the ship at a depth of about 8¼ ft. below the surface of the water. The launch carrying the spar was, as will be remembered, one supplied to the French Government by Messrs. Thornycroft & Co., and it attacked the Bayonnaise at a speed of about 14 knots per hour, the frigate itself being towed by a steamer at a rate of about 6 knots per hour. Just before the moment of striking it was found necessary to reduce the speed of the launch. The explosion was followed by a shock, and a large wave, which flooded the launch and drove it back for a considerable distance. The damage done to the Bayonnaise was, as will be seen from the engravings, of such a nature as to have sent her to the bottom immediately, had not the precaution been previously taken of loading her with empty casks. She was shortly after towed into dock for inspection.—*Engineering.*

THE CLOCK MAKER'S HAND TURNING TOOL.

THE tool shown in the accompanying illustration is believed to have originated among the clock factories of New England, yet has been in use so long that no very definite history of its origin can be obtained. Until recently its use was seldom seen beyond the vicinity of clock and watch factories, but now other manufactories begin to realize its value, and its employment no doubt will become more extended and more general.

Fig. 1.



By means of this tool pieces of work may be produced in duplicate, exact in diameter, length and form, and this can be readily done without the use of calipers or gauges.

By a very simple arrangement the tool can be used on almost any hand lathe and made to operate as a special tool upon a very wide range of sma l work.

The device consists of a stock or holder as shown in Fig. 1—the middle of which denoted by A is square and contains three or four square slots with a set screw to each slot to hold different turning tools. Each end of the stock is turned parallel as denoted by B, C. In Figs 1 and 2, D, E, and F are the tools, and G, H, are the set screws.

Fig. 2.

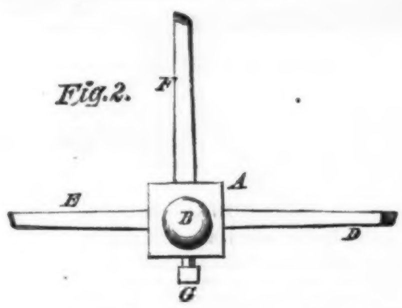


Fig. 3 represents top and side view of a plate, of which there must be two, one to fasten on the headstock and one on the tailstock of the lathe as shown in Fig. 4. In Fig. 5 the manner of using the tool is shown, similar letters of reference denoting similar parts in all the figures.

The plates P P are bolted by the screws I I, J J, to the head block H and the tail stock T of the lathe. The tool holder is placed so that the cylindrical ends B C rest on the ends of these plates and in the angles p' p'. The cutting tool D is sustained, as shown upon the lathe rest R. In use the operator holds the stock A in his hands in the most convenient manner, using the tool E as a handle when there is a tool in the position of E. The cutting point of the tool is pressed up to the work W, and the feed is carried along by hand. It is obvious, however, that when the cylindrical ends B C of the holder come against the shoulders o o of the plates P P, the tool cannot approach any nearer to the diametrical center of the work hence the diameter to which the tool will turn is determined by the distance of the shoulders O of the plates P from the center of the lathe centers as shown in Fig. 1 by the line L. In carrying the cut along it is also obvious that the lateral travel of the stock or holder must end when the end of the square part A comes against the side face of either of the plates. In our engraving we have shown the tool D cutting a groove in the work W, while the shoulder of the holder is against the plate fastened to the lathe tail stock T; and so long as the operator, in each case, keeps the shoulder against that plate, the grooves upon each piece of work will be cut in the same position, for it will be observed that the position in t e length of the work performed by each tool is determined by the distance of the cutting part of each tool from the end of the square part A of the tool holder. All that is necessary then is to adjust each tool so that it projects the proper distance to turn the requisite diameter and stands the required distance from the sh ulders of the square to cut to the desired length, and when once set error cannot occur.

Fig. 3.

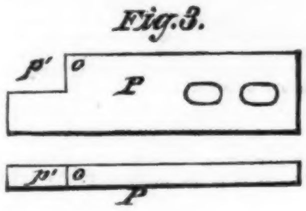
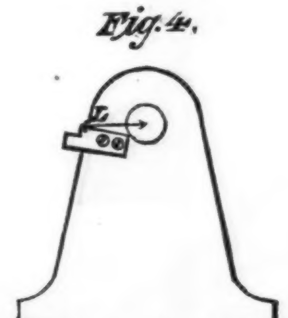
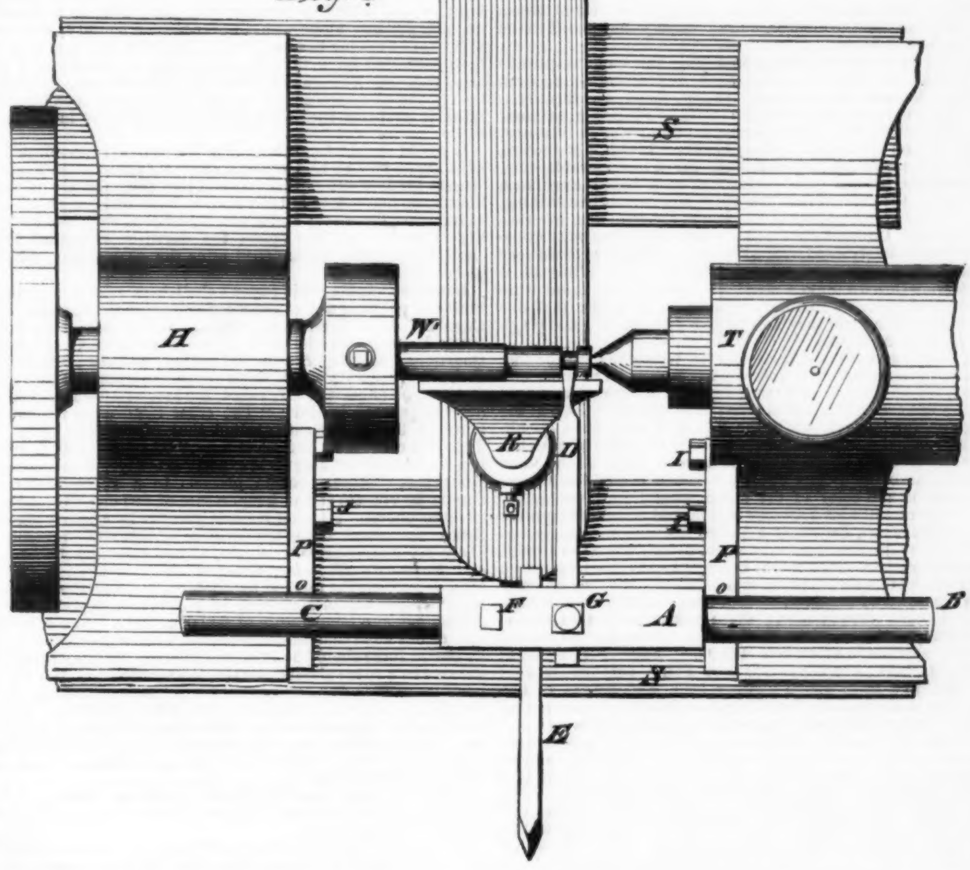


Fig. 4.



This plain description of the device, however, does not convey an adequate idea of its importance. Suppose, for example, that it is required to turn a number of duplicate pieces, each with a certain taper. All that is necessary is to adjust the plates P in their distances from the lathe centers. If the large end of the taper, on the work, is required to stand nearest the lathe head stock H, the plate P on t e head stock must be moved until its shoulder O is farther from the lathe center. If, however, the work requires to be made parallel the plates P must be set the same distance for the axial line of the cent rs. If it be desired to have a parallel and a taper in proximity upon the same piece of work the tool must have one of its cylindrical ends taper and use it upon the taper part of the work. All kinds of irregular work may be performed by varying the form of the cylindrical ends of the tool holder. In this event the shoulder O of the plates P should be made V-shaped and of steel and hardened.

Fig. 5



A GOOD alloy for journal boxes is made of copper, twenty-four parts; tin, twenty-four parts; and antimony, eight parts. Melt the copper first, then add the tin, and lastly the antimony. It should be first run into ingots, then melted and cast in the form required for the boxes.

SOME small shipments of coal have been received in San Francisco from a mine near Oonalaska, Alaska, and a company has been organized to work the mines.

THE ELASTICITY AND DENSITY OF VAPORS.

*Experiments on the Maximum Elasticity and Density of Vapors.**

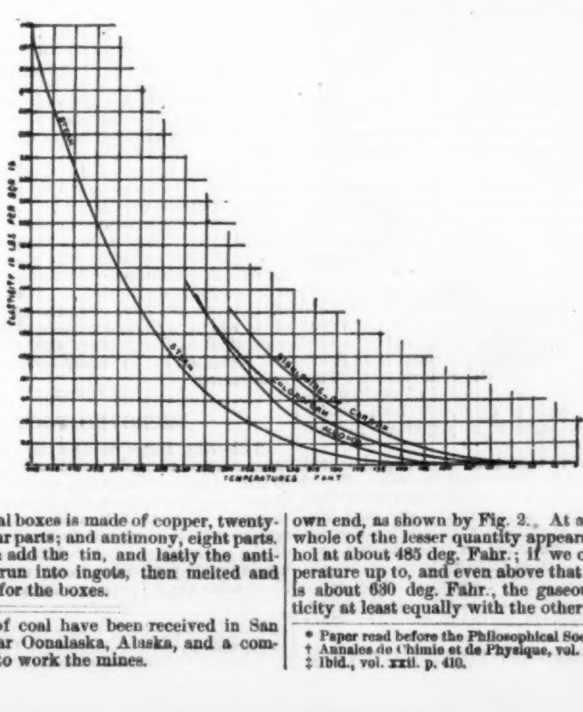
By Mr. ALEX. MORTON.

(Concluded from SUPPLEMENT No. 86, p. 1365.)

CAGNIARD DE LA TOUR performed his experiments with tubes heated in an oil bath;† and to ascertain when the liquids had become gaseous, he must have raised the tube frequently, so as to examine minutely if the liquid had vanished, because oil darkens in color with heat, and he speaks of a handle to each tube. This action would cool the tube, causing condensation, and so far decompose the glass, before the experiment had been performed, as to make the point uncertain. Whilst experimenting with his tubes containing distilled water, he does not say what was the substance he used as a bath; but with any substance, if ever the experimental tube fell in temperature, its transparency would have been so far lost. This difficulty vanishes when the experimental tube is constantly surrounded by a body of a considerably higher temperature than itself.

His experiments with ether and bisulphide of carbon given in the following year, 1823,‡ show, I think, that with the greater quantities of liquid the curve representing the elasticity continues undisturbed after the liquid has passed into this state. As a further test I have experimented with a U-tube similar to those I have described near the beginning of this paper, and represented by Figs. 1 and 2; but both ends of the tube now contain the same liquid, the one end rather less in quantity than the other, but filling at least about one-half of the spaces.

In experimenting with these, the end containing the least quantity of liquid generally boils first; if not, it can be made to do so, which compresses the greater quantity up into its own end, as shown by Fig. 2. At a certain temperature the whole of the lesser quantity appears gaseous, and for alcohol at about 485 deg. Fahr.; if we continue to raise the temperature up to, and even above that of melting lead, which is about 630 deg. Fahr., the gaseous end increases in elasticity at least equally with the other end, because it continues



* Paper read before the Philosophical Society of Glasgow.
† Annales de Chimie et de Physique, vol. xxi. p. 181.
‡ Ibid., vol. xxii. p. 410.

to compress the liquid in that end, and prevents it from evaporating. In cooling down again, the gaseous end still holds good until the liquid is seen to form in its own end after the cloudy state has passed. By adjusting the gas flame so as to have a sudden command of the heat in the bath, you may then catch in both ends the boiling liquid when the mercury is level in both limbs. With three single tubes containing sulphurous acid of commerce (H_2SO_3) they go into the state in the following order: That containing one-fourth of liquid first, at about the temperature of melting lead; that containing one-third next, at, I should say, less than 10 deg. more; and lastly, that containing one half, at about 20 deg. more. These temperatures are, of course, merely approximate from the time after the lead melts, compared with the rate of heating at rather lower temperatures. This substance, somewhat resembling the distilled water, alters the transparency of the glass at the higher temperatures, probably from the same cause, and that tube containing the least quantity of liquid loses its transparency first, which is another instance of the vapor being more effective in obscuring the glass than the liquid itself. This substance should be quickly heated up to the required temperature, which is a little above that of melting lead. You can with this substance repeat the process with the same tubes four or five times at least, and still the tubes will have sufficient transparency to note the change, and invariably they go off in the order I have stated.

With three single tubes containing the same respective quantities of liquid benzole, that containing one-fourth attains the state first, at about 610 deg. Fahr., and those containing one-third and one-half very nearly together, at about 620 deg. Fahr., but still below the boiling point of mercury, near that of melting lead; the cloud is a bluish, dirty white, and can easily be kept constant for a considerable time. These experiments were performed on a bench close to a window, through which the clear sun was often shining. The tubes were hung between me and the sun, and his rays on passing through the vapor, when the tubes were apparently completely filled with a homogeneous gas, showed very distinctly the agitation going on within them. When these rays were thrown upon a sheet of white paper, there were lights and shadows so different in depth of shade, that one would have thought they had passed through a greatly agitated cloud of smoke.

I have been a little particular in trying to describe that point when a cloud does actually form within the tube, and the color of it for the different vapors I have experimented with; but the shadows just referred to were produced at the higher temperatures, when the vapor, viewed in a certain light, seemed pure and transparent. My object in describing the special color of the cloudy state is to draw attention to this means of analyzing liquids. When I could tell from the motion and color of the cloud what liquor was under experiment, without knowing beforehand either the liquid or the temperature, I think if experiments were so arranged as to have, as it were, spectra of these clouds when the liquids are in that state, much might be gained as to the constitution of both liquids and vapors; and I find single tubes containing one-third of liquid at 60 deg. Fahr. are the best for observing this phenomenon; these attain that state in the following order:—ether at 405 deg. Fahr., alcohol at 490 Fahr., bisulphide of carbon at 500 deg. Fahr., chloroform at 520 deg. Fahr., benzole at 620 deg. Fahr. sulphurous acid (H_2SO_3) at a little above melting lead, and spirits of turpentine at about that of melting zinc, but much below that of antimony—thus nearly agreeing with distilled water.

Many single tubes were purposely raised in temperature until they burst; and the arrangement for this purpose consisted in laying one tube at a time on a cradle, D, of thin sheet copper, at an angle in the bottom of the bath A. Each tube was surrounded with a spiral cutting of sheet-lead and sheet-zinc, and a small piece of antimony was suspended in contact with the tube under experiment; these were my thermometers. The cover of the bath was generally a piece of common sheet-glass, but at times merely a light card. One tube, containing one-half of liquid chloroform, was laid on a cradle, and the bath covered with a loose card. It appeared to be completely filled with a homogeneous vapor, at 520 deg. Fahr., and exploded with great violence after the zinc had melted; and although the bath was merely covered with a loose card, which I had removed to ascertain if the zinc had melted, and replaced just the moment before, the bottom was driven out of the brass bath, the card riddled with fine holes, and the cradle as flat as if it had been crushed under a heavy roller, and the tube itself glass dust.

The effects were so great compared with a simple burst, that nothing but an actual explosion seemed to explain them. Having several other tubes exactly similar, and containing some of the very same sample of chloroform, I tried to catch these just before they had sufficient time or heat to explode them. I lost many, but have preserved some. These have been considerably above the temperature of melting zinc, I should say near to that of antimony; and the liquid, I find, shows evident signs of decomposition, as the liquid in one is now so dark brown as to appear nearly black. I have also preserved some tubes containing liquid alcohol absolute, which have several times been raised to an equally high temperature, still the liquid appears in these as pure and transparent as originally; but on trying these and the colored chloroform suspended in the bath, as at first, I found they had lost their power of ebullition.

Two tubes, one containing one-fourth of the brown chloroform, and the other one-fourth of the highly heated alcohol, when hung in the bath these liquids expand quietly until the whole tubes are completely filled, showing but the faintest attempt at ebullition at a mean temperature; but above and below that there are no signs of ebullition whatever. That which contains the alcohol when expanded to about three parts full, although viewed with a powerful lens, you cannot detect the difference between what is liquid and the vapor above it—both have the same curling motion; but you can, in some lights, easily detect a hair line of demarcation rising gradually to the top of the tube as the heat augments. This line is more easily detected with the colored chloroform, as the vapor has rather less depth of color, but both liquids seem to follow the same law—that is, they increase in bulk without the boiling action.

These very same tubes have repeatedly been experimented with, and often raised in temperature until the whole liquid had been converted into vapor, and on having been cooled down again, the liquids appeared perfectly pure and transparent as at first. At first, and at every re-heating, they invariably began to boil, and continued so to do as the heat increased, the line of demarcation falling gradually towards the bottom, until the last globule had disappeared by evaporation; and what is very remarkable, at about that temperature when the one disappears at the bottom the other disappears at the top.

What I wish to call attention to, is the fact that the original liquid was converted into vapor by the apparent action of ebullition, whilst it now simply expands into vapor. What, therefore, is the action of ebullition, and wherein do the fluids differ?

Liquids in bent tubes, such as Figs. 1 and 2, sealed cold—that is, containing air in the spaces—go into the "Cagniard de la Tour" state at a few degrees lower temperature than those in single tubes, such as Figs. 4, 5 and 6, sealed cold; and those single tubes, sealed cold, at a lower temperature than those which were sealed when thoroughly boiling. From this it would appear that the increased pressure from the vapor of mercury and air assists in inducing this state at the higher temperatures in the bent tubes, and that the pressure of the air alone assists in the single tubes sealed cold. Cagniard de la Tour's later experiments with ether in bent tubes containing mercury and air, show 10 deg. Réaumur less heat. If, therefore, pressure alone can induce this state with liquids considerably removed from the temperature of maximum density as liquids, then it would follow that, if the pressure be great enough, this state would be induced at much lower temperatures; and my experiments with single tubes containing three fourths of liquids, I think, confirm this.

If one were to continue this line of thought, it would tend to the assumption that, under a certain pressure, every liquid would be in this peculiar state, and that, as a consequence, pressure alone would never induce solidity; and we know that Faraday has said, "Solidification has not as yet been conferred on a fluid by any degree of pressure;"* as also, that Sir William Thomson has proved that pressure lowers the temperature of solidifying water; therefore I think it probable that pressure alone may never induce solidity.

It would appear from these experiments that those liquids most expansible by heat arrive at the "Cagniard de la Tour" state at the lowest temperature, and that the volatility of the liquids, or the weight of their vapors, have little or no effect in inducing this state; and if the liquid be of sufficient quantity in relation to the space, the curve representing the elasticity remains undisturbed after the change has taken place.

Before concluding this paper I shall here take the opportunity of pointing out an error regarding the temperature of Cagniard de la Tour's experiments, which has crept into every English translation I have come across. In the *Quarterly Journal of Science, Literature, and the Arts*, London, 1823, vol. xv., p. 145, there appears a translation of his first memoir; and at p. 147, par. 4, the temperature is given in degrees Fahrenheit, thus: "The ether required a temperature of 320 deg. Fahr, alcohol that of 405 deg., Fahr." Thomson on "Heat and Electricity," Glasgow, 1830, p. 220, in a foot-note describing these experiments says, in the second paragraph, "Ether became gaseous in a space scarcely double its volume at the temperature of 320 deg.; pressure, 38 atmospheres. Alcohol became gaseous in a space about thrice its volume at the temperature of 404½ deg.; pressure, about 129 atmospheres." Again, in the *Practical Mechanic and Engineer's Magazine*, Glasgow, 1842, vol. i., p. 205, after describing the method he adopted, we find the following: "Ether becomes gaseous in a space scarcely double its volume, at a temperature of 320 deg. and exerted a pressure of not more than 38 atmospheres, whereas by calculation its elastic force should be 168 atmospheres. Alcohol became gaseous in a space about three times its volume, at the temperature of 404½ deg., and exerted a pressure of only 139 atmospheres, whereas calculation gives 221 atmospheres. Faraday, in his "Researches in Chemistry and Physics," p. 126, gives no quotations as to temperature, but he refers to the *Quarterly Journal*. This is nearly as misleading as if he had had quoted therefrom.

There are other English books equally at fault; and it seems to me that the *Quarterly Journal* has been the leader they have all followed.

On making my experiments with these liquids, I found they did not even come near to this in regard to temperature, and on referring to his original paper I at once discovered the error of our English translations. My friend, M. Bouzier, who is a teacher of French here, has given me, to the best of his ability, a correct translation of the whole paragraph referred to, which runs thus:

"To determine the degree of heat to which ether and alcohol reduced into vapor, the tubes containing those liquids were heated in an oil bath, in which a mercury Réaumur thermometer was plunged. The oil was put into a glass cylindrical vase or gauge. By this means the moment when the liquids in the tubes reduced into vapor was easily noticed; and it was ascertained that ether required for that reduction 160 deg. of heat, and alcohol 207 deg."†

From this it is evident that Cagniard de la Tour had used a mercurial thermometer graduated to the scale of Réaumur, which has only 80 parts between the freezing and the boiling points of water; therefore 160 deg. Réaumur should read 392 deg. Fahr, instead of 320 deg. Fahr.; and 207 deg. Réaumur should read 497·75 deg. Fahr., instead of 404½ deg. Fahr. The error has arisen from assuming that the thermometer he experimented with had been graduated to the scale of Centigrade, whereas he distinctly says Réaumur. When this error of nearly 100 deg. Fahr. is corrected, his experiments and mine with these liquids become very near alike.

The pressure of the vapor of alcohol given in the original paper, and in the *Quarterly Journal*, is 119 atmospheres, therefore the numbers 129 and 139 atmospheres are evidently errors also. In his second Memoir,‡ already referred to, his tables giving the elasticity of ether and bisulphide of carbon at different temperatures have the column of temperatures headed thus: "Degrés de Réaumur." There can therefore be no question regarding this point, and that the whole of his experiments were performed and registered with a Réaumur mercurial thermometer. It is rather strange that these experiments, so much referred to by many eminent physicists, which, although translated and published in London more than half a century ago, should have been copied and reprinted in so many books containing such errors unobserved.

IMPROVED FOOT BELLOWS.

In blowing forge fires by the old-fashioned bellows, one hand rests on the pole continually, and is always occupied, so that to replenish the fire, or do any little office that requires special attention, the blast must stop and time be lost thereby. Not only is this the fact, but the common bellows require muscle to operate them all day; whereas the weight of the workman does the business in these, leaving both hands free to perform any duty they may find to do. In the illustration the bellows are shown set end to end on a platform, and connected by a strap, so that when one is in a state of collapse the other is full, thereby a continuous current is kept up at all times. The action of this apparatus

IMPROVED FOOT BELLOWS.

is carried on by simply rocking the body from side to side, thus throwing the weight off from one foot to the other. For very many purposes it will be found a useful substitute for the old mode of operating bellows.

* Faraday's "Researches in Chemistry and Physics," p. 99.
† "Annales de Chimie et de Physique," vol. xxi., p. 181, par. 3.
‡ "Annales de Chimie et de Physique," vol. xxii., p. 410.

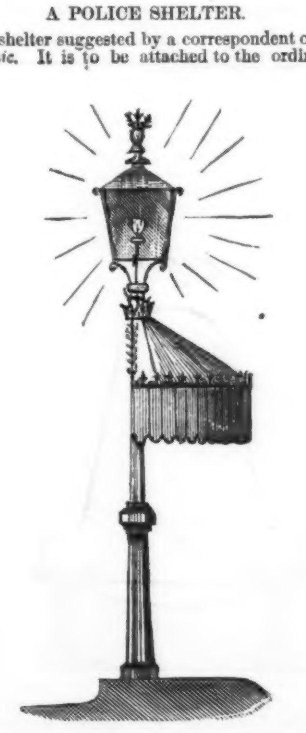
IRISATION OF GLASS.

By E. Fremy and Clemandot.

Glass submitted to influences which very slowly effect its decomposition, such as exposure to continued moisture; to exhalations arising from damp earth; to ammoniacal or acid vapors; often becomes covered with a thin film or scale, which produces very remarkable phenomena of irisation. The authors have, after many experiments, succeeded in imitating this effect, and have produced glass with an adherent coating, resembling nacre or mother-o'-pearl, by submitting the glass, under the influence of heat and pressure, to the action of water containing 15 per cent of hydrochloric acid. The conditions necessary to ensure success, the composition of the external film, the alterations produced in it by different chemical reagents, etc., will be described in a subsequent communication. They are of opinion that the facility with which glass undergoes irisation, that is to say, the facility with which it is attacked, should be studied by those who manufacture glass for special objects, and they suggest that such glass should be qualitatively examined in the manner above mentioned.

A POLICE SHELTER.

This is a shelter suggested by a correspondent of the English *Mechanic*. It is to be attached to the ordinary lamp-



post, semi-circular in shape, having a glass to admit light; the back, if necessary, could be brought down to the ground and slightly curved, forming a kind of half sentry box.

[NATURE.]

HOW TO DRAW A STRAIGHT LINE.*

IV.

I NOW come to the second of the parallel motions I said I would show you. If I take a kite and pivot the blunt end to the fixed base and make the sharp end move up and down in a straight line, passing through the fixed pivot, the short links will rotate about the fixed pivot with equal velocities in opposite directions; and, conversely, if the links rotate with equal velocity in opposite directions, the path of the sharp end will be a straight line, and the same will hold good if instead of the short links being pivoted to the same point they are pivoted to different ones.

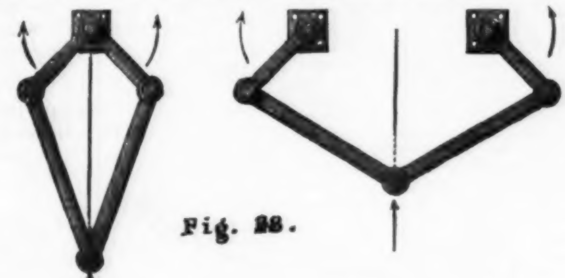


Fig. 28.

To find a linkwork which should make two links rotate with equal velocities in opposite directions was one of the first problems I set myself to solve. There was no difficulty in making two links rotate with equal velocities in the same direction—the ordinary parallelogrammatic linkwork employed in locomotive engines, composed of the engine, the two cranks, and the connecting rod, furnished that; and there was none in making two links rotate in opposite directions with varying velocity; the contra-parallelogram gave that· but the required linkwork had to be discovered. After some trouble I succeeded in obtaining it by a combination of a large and small contra-parallelogram put together just as the two kites were in the linkage of Fig. 18. One contra-parallelogram is made twice as large as the other, and the long links of each are twice as long as the short.

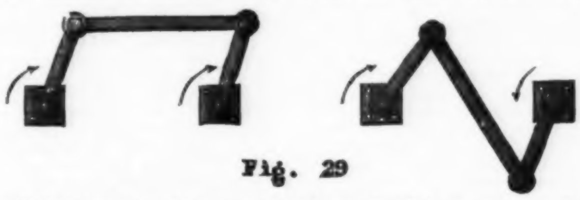


Fig. 29

The linkworks in Figs. 30 and 31 will, by considering the thin line drawn through the fixed pivots in each as a link, be seen to be formed by fixing different links of the same six-link linkage composed of two contra-parallelograms as just stated. The pointed links rotate with equal velocity in opposite directions, and thus, as shown in Fig. 28, at once give parallel motions. They can of course, however, be usefully employed for the mere purpose of reversing angular velocity.

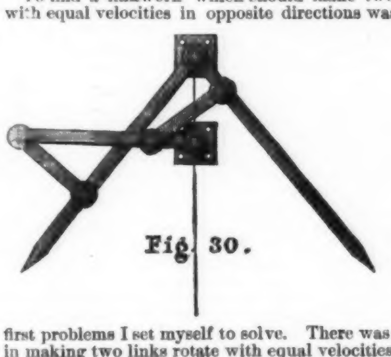


Fig. 30.

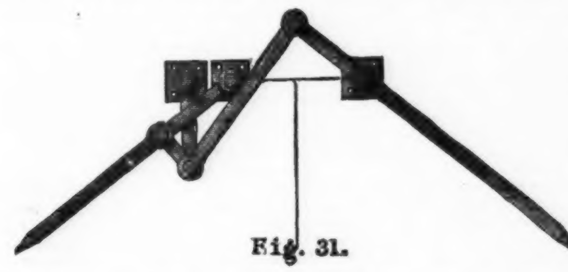


Fig. 31.

An extension of the linkage employed in these two last figures gives us an apparatus of considerable interest. If I take another linkage contra-parallelogram of half the size of the smaller one and fit it to the smaller exactly as I fitted the smaller to the larger, I get the eight-linkage of Fig. 32. It has, you see, four pointed links radiating from a center at equal angles; if I open out the two extreme ones to any direct angle, you will see that the two intermediate ones will exactly *trisect the angle*. Thus the power we have had to call into operation in order to effect Euclid's first postulate—linkages—enables us to solve a problem which has no "geometrical" solution. I could of course go on extending my linkage and get others which would divide an angle into any number of equal parts. It is obvious that these same linkages can also be employed as linkworks for doubling, trebling, etc., angular velocity.

Fig. 32.

Fig. 33

Another form of "Isoklinostat," for so the apparatus is termed by Prof. Sylvester, was discovered by him. The construction is apparent from Fig. 33. It has the great advantage of being composed of links having only two pivot distances bearing any proportion to each other, but it has a larger number of links than the other, and as the opening out of the links is limited, it cannot be employed for multiplying angular motion.

Subsequently to the publication of the paper which contained an account of these linkworks of mine of which I have been speaking, I pointed out, in a paper read before the Royal Society, that the parallel motions given there were, as well as those of M. Peaucellier and Mr. Hart, all particular cases of linkworks of a very general character, all of which depended on the employment of a linkage composed of two similar figures. I have not sufficient time, and I think the subject would not be sufficiently inviting on account of its mathematical character, to dwell on it here, so I will leave those in whom an interest in the question has been excited to consider the original paper.

At this point the problem of the production of straight-line motions now stands, and I think you will be of opinion that we hardly, for practical purposes, want to go much farther into the theoretical part of the question. The results that have been obtained must now be left to the mechanician to be dealt with, if they are any practical value. I have, as far as what I have undertaken to bring before you to-day is concerned, come to the end of my tether. I have shown you that we *can* describe a straight line, and *how* we can, and the consideration of the problem has led us to investigate some important pieces of apparatus. But I hope that this is not all. I hope that I have shown you (and your attention makes that hope a belief) that this new field of investigation is one possessing great interest and importance. Mathematicians have no doubt done much more than I have been able to show you today, but the unexplored fields are still vast, and the earnest investigator can hardly fail to make new discoveries. I hope therefore that you whose duty it is to extend the domain of science will not let the subject drop with the close of this my lecture.

* Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A. Concluded from p. 1365.

CHEMISTRY OF THE BARKS OF THE OAK, WILLOW, AND ELM.

By E. JOHANSEN.

THE investigation was undertaken with the view of ascertaining the nature of the different tannin-like substances contained in the barks of the oak, willow, and elm, and it was hoped, by isolating these and carefully examining their properties and the nature of their principal compounds, to ascertain whether they were analogous or even identical. By a long and elaborate process, the different tannins were separated from the three barks in something like a pure state.

Oak Tannin is a red-brown amorphous glistening body, easily soluble in alcohol, slightly soluble in ether, and forms an imperfectly clear solution in water. In its behavior to litmus paper, metallic salts, and alkaloids, it is completely analogous to gallotannic acid. Dried at 110°, it lost 8.48 per cent of water. On analysis, it gave 54·61 per cent of carbon, 5·82 per cent of hydrogen, and 40·07 per cent of oxygen, agreeing approximately with Wagner's formula, $C_{14}H_{16}O_9$, which requires 53·85 per cent of carbon, and 5·13 per cent of hydrogen. It contains also 0·77 per cent of nitrogen, and 0·13 per cent of ash.

Willow Tannin consists of a brown-red amorphous body, with a slightly astringent taste; easily soluble in alcohol, slightly soluble in ether, and forming a thick solution with water. With ferric salts it gives a deep black color, turned violet-red by alkalies. It precipitates mercuric nitrate and chloride, and zinc and copper sulphates, as well as albumen, starch, and alkaloids. At 120° the willow tannin lost 10·10 per cent of water, and on analysis gave 51·13 per cent of carbon, 4·78 per cent of hydrogen, and 44·09 per cent of oxygen. It contains also 1·88 per cent of nitrogen and 1·63 per cent of ash. Another specimen, prepared in a different manner, though possessing the same reactions as the last, contained 51·26 per cent of carbon and 5·99 per cent of hydrogen, besides having independently 0·44 per cent of nitrogen and 1·42 per cent of ash.

Elm Tannin.—In appearance and solubility this variety resembles oak tannin. With ferric chloride, it gives a dirty-green precipitate, turned violet-red by sodium hydrate. With ferrous sulphate, it gives a pure green precipitate. It precipitates lead and copper acetates, and zinc sulphate after some time. With zinc chloride, mercuric nitrate, calcium acetate, etc., it gave the usual reactions. At 110° elm tannin loses 3·32 per cent of water, and, on analysis, gives 44·54 per cent of carbon, 4·72 per cent of hydrogen, and 50·74 per cent of oxygen, besides containing 1·21 per cent of ash.

The salts of these three tannin acids (quercitannic, salitannic, and ulmotannic) were next examined.

Lead Salts.—Quercitannate of lead is a chocolate-brown, amorphous mass, slightly soluble in water, insoluble in alcohol or ether. On heating it to 110°, it lost 9·66 per cent of water; and on analysis it gave 22·85 per cent of carbon, 1·47 per cent of hydrogen, 9·14 per cent of oxygen, and 36·54 per cent of lead oxide. The salitannate of lead resembled the last body, and on drying at 120° lost 4·50 per cent of water, and on analysis gave 22·53 per cent of carbon, 1·35 per cent of hydrogen, and 53·28 per cent of lead oxide. By fractionally precipitating with a lead salt, both these acids gave salts of varying constitution. Ulmotannate of lead was greyer than the last body; and on analysis gave 21·36 per cent of carbon, 1·51 per cent of hydrogen, 10·32 per cent of oxygen, and 66·81 per cent of lead oxide.

Copper Salts.—Quercitannate of copper is a brown substance, insoluble in alcohol and ether, and sparingly soluble in water. At 110° it lost 12·23 per cent of moisture, and on analysis gave 39·99 per cent of carbon, 2·38 per cent of hydrogen, 28·14 per cent of oxygen, and 29·49 per cent of copper oxide. Salitannate of copper forms a dark reddish-brown salt, which lost at 120° 12·4 per cent of moisture; and on analysis gave 39·36 per cent of carbon, 2·35 per cent of hydrogen, 27·83 per cent of oxygen, and 30·46 per cent of copper oxide. Ulmotannate of copper is chocolate-brown, and after drying at 110° gave 39·68 per cent of carbon, 1·93 per cent of hydrogen, 17·98 per cent of oxygen acid, and 40·41 per cent of copper oxide.

Tin Salts—Quercitannate of tin is a greenish-brown substance, insoluble in alcohol and ether, and only sparingly soluble in water. At 110° it loses 5·98 per cent of moisture, and on analysis gave 36·32 per cent of carbon, 2·56 per cent of hydrogen, 20·69 per cent of oxygen, and 40·43 per cent of stannous oxide. The formula $C_{30}H_{26}O_{13}.3SnO$ agrees fairly with these numbers. Salitannate of tin is a chocolate-colored body, which loses 7·18 per cent of moisture at 120°, and on analysis gives 35·17 per cent of carbon, 2·79 per cent of hydrogen, 15·05 per cent of oxygen, and 46·60 per cent of stannous oxide. Ulmotannate of tin on drying at 110°

gave 38·90 per cent of carbon, 2·40 per cent of hydrogen, 13·66 per cent of oxygen, and 44·95 per cent of stannous oxide.

When these different tannins were acted on by dilute acids in the usual manner, as Grabowski has already shown, the oak tannin yields an easily decomposed saccharide and a crystalline body. The amount of these bodies obtained varies with the strength of acid employed. On purification the saccharide is obtained as a brown substance, forming a dark-brown bitter syrup. Similar bodies were obtained from the willow tannin. On analyses the saccharide obtained from the willow tannin gave 36·94 per cent of carbon, 5·19 per cent of hydrogen, and 57·87 per cent of oxygen. Elm tannin, on the contrary, yields no crystalline body, but only a saccharide resembling in every respect the last.

On fusing with potassium hydrate, the oak tannin yields, amongst other products, butyric acid amongst the volatile products, and protocatechuic acid from the residue. Willow tannin, similarly treated, yielded acetic and butyric acid amongst the volatile products, whilst the residue in the retort contained a body whose identity could not be satisfactorily made out. Elm tannin, treated in the same manner, yielded acetic and butyric acids among the volatile products, and oxyphenic acid in the residue.

SPONGY IRON AND PUTRESCENT ORGANIC MATTER.*

By GUSTAV BISCHOF.

IT is indeed fortunate that the smell and taste are generally extremely sensitive indicators of putrefaction in articles of food. This does not, however, apply to drinking water, which may be largely polluted by putrescent organic impurities without causing any suspicion to our senses. And yet the question of the wholesomeness of water hinges mainly upon the presence or absence of such putrescent matters, as they themselves are the cause of derangements of the human system. Most serious, however, are the consequences when those low forms of organic life, which in all probability form the specific poison of cholera, typhoid fever, and other diseases, gain admission to drinking water polluted by putrescent matter.

A number of observations point to the conclusion that these organisms, or their germs, are not infectious as long as surrounded by fresh organic matter; but as soon as fermentation sets in, they show their poisonous virulence. Thus, it has been observed that the discharges of cholera and typhoid patients are not infectious as long as they are fresh, but by putrescence their poisonous character is developed.

Chemical analysis is incapable of discriminating between living or dead, fresh or putrescent organic matters. The microscope reveals the nature more fully; but it is nevertheless frequently a matter of great difficulty to decide as to the existence or non-existence of bacteria of putrefaction, or their germs, in water. It thus appeared to me that this information might, in some cases at least, be gained with greater certainty by an indirect method.

If we want to determine whether a gas be carbonic anhydride, we pass it through potash bulbs, and see whether these increase in weight. Similarly the presence or absence of

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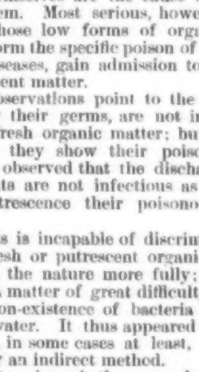


a

s s

putrefactive agencies in water may be determined by their action upon organic matter. The test I selected is fresh meat, as the slightest putrescent changes in it can most readily be detected by its smell.

The experiments, which were made with a view of determining the improvement of water by certain filtering media, were, with the exception of experiment VIII., carried out in the following manner :—

On to the perforated bottle, *a*, of a stoneware vessel, *s s*, I place some fresh meat. The vessel is then filled to about two-thirds with the materials to be experimented upon, and lastly, with water. In opening, *c*, a tin tube is fixed, which is first bent upwards and then downwards in the shape of an inverted U, to prevent any bacteria or their germs from passing through this outlet-tube into the bottom of the vessel. The air-pipe, *d*, down to *c*, is filled with firmly-compressed cotton wool, and a glass tube, scaled at its bottom, passed down through the material experimented upon, to allow of the temperature being measured in close proximity to the meat. The vessels thus prepared are immersed in a boiler filled with cold water, which is gradually heated and kept boiling for several hours. The object of this is to destroy any germs adhering to the meat. The temperature at the bottom of the sealed glass tube was, during the boiling, in each of the following experiments 93-95° C.

After cooling, the Chelsea Company's water was constantly passed through the vessels in the direction indicated by arrows, at as nearly as possible a uniform speed.

It is thus evident that any bacteria of putrefaction, or their germs, in the water would, after a time, render the meat putrid, or, if it remains fresh, they must have been absent, or at least inactive, when the water reached the meat.

I now proceed to describe the experiments :—

EXPERIMENT I.

One of the vessels was filled with spongy (metallic) iron, and treated as before described ; after a fortnight the meat was perfectly fresh.

EXPERIMENT II.

A vessel filled with animal charcoal; after a fortnight the meat showed strong evidence of incipient putrefaction. As experiments I. and II. were conducted side by side, this result proves that the preservation of the meat in experiment I. was not due to any external cause, such as the low temperature then prevailing.

* Read before the Royal Society, April 19th, 1877.

EXPERIMENT III.

Water continuously passed through a vessel filled with spongy iron for four weeks ; even then the meat was perfectly fresh and hard.

EXPERIMENT IV.

was a repetition of II., the filtration of water through animal charcoal being continued for four weeks. The meat was soft and quite putrid. In the course of this experiment the exit-tube was several times choked by mucous matter

EXPERIMENT V.

In experiments I. and III. with spongy iron, this material was employed without separating any of the fine dust. In order to ascertain whether bacteria were merely mechanically retained, a vessel was charged with spongy iron, from which all the finer particles had been separated by a sieve with thirty holes on the linear inch. The filtering medium in this case was therefore of a porous nature. After four weeks' filtration the meat was perfectly fresh.

EXPERIMENT VI.

In the previous experiments with spongy iron the meat was in contact with water, from which the iron in solution had not been separated. With a view of ascertaining whether the iron in solution was the preserving agent, a stoneware vessel was charged under the spongy iron with pyrolusite and sand, so as to abstract the iron from the water before it came in contact with the meat. After four weeks' filtration the latter was found perfectly fresh.

EXPERIMENT VII.

By a separate experiment I ascertained that the oxygen is completely abstracted from water during its passage through spongy iron. In order to determine whether the absence of oxygen be the cause of the preservation of the meat, and whether the bacteria or their germs be killed or can be revived when supplied with oxygen, an evaporating basin was inverted over the meat. This must have retained a quantity of air in its cavity, the air being gradually dissolved by the water in close proximity to the meat. After four weeks' filtration the meat was perfectly fresh ; I succeeded in collecting a small bubble of the gas, still in the cavity of the evaporating basin. This was quite free from oxygen.

It it therefore doubtful whether oxygen was supplied to the water sufficiently long to justify any conclusions from this experiment. However, the result of experiment VIII. rendered a repetition unnecessary.

EXPERIMENT VIII.

Fresh meat was placed at the bottom of a glass vessel and left standing, covered with about 4 inches of spongy iron and water. The vessel in this instance was not boiled. After three weeks the meat was very bad, demonstrating that the action of the bacteria of putrefaction adhering to the meat was not prevented by the spongy iron above, and if, during the previous experiments with spongy iron, agencies capable of causing putrefaction had at any time come in contact with the meat—in other words, if the bacteria had not been killed in their passage through spongy iron—the meat must, as in this last experiment, have shown marks of their action. It therefore appears that bacteria are rendered harmless when passing in water through spongy iron. This conclusion is further corroborated by the observation that even effluent sewage-water, after passing through the spongy material, has remained perfectly bright for now five years, when exposed to light in a half-filled stoppered bottle.

I believe that the action of spongy iron on organic matter largely consists in a reduction of ferric hydrate by organic impurities in water. We know that even such organic matter as straw or branches is capable of reducing ferric to ferrous hydrate. We know that even such indestructible organic matter as linen and cotton fibres is gradually destroyed by rust stains. This action is slow when experimenting upon ordinary ferric hydrate, but it may, *in statu nascenti*, be very energetic—the more so, if we consider the nature of the organic matter in water. Ferric hydrate is always formed in the upper part of a layer of spongy iron, when water is passed through that material. The ferrous hydrate resulting from the reduction by organic matter may be re-oxidized by oxygen dissolved in water, and thus the two reactions repeat themselves. This would explain why the action of the spongy iron continues so long.

It is, however, quite certain that there is also a reducing action taking place when ordinary water is passed through spongy iron. This is clearly indicated by the reduction of nitrates.

Our knowledge of those low organisms which are believed to be the cause of certain epidemics is as yet too limited to allow of direct experiments upon them. It is not improbable that, like the bacteria of putrefaction, they are rendered harmless when water containing them passes through spongy iron ; but until we possess the means of isolating these organisms, this question can only be definitely settled by practical experience. Should not this be satisfactory, should those specific contagia not be destroyed when passing in water through spongy iron, then the separation of bacteria by spongy iron may afford means of isolating those germs of disease ; should it be favorable, then we shall have found in spongy iron the material to prevent the spreading of epidemics by potable water.

NEW DERIVATIVE OF ALBUMINOID SUBSTANCES.

By P. SCHUTZENBERGER.

THE liquid resulting from the action on albumen of solution of baryta at 130° was precipitated by carbonic acid, filtered and concentrated. Crystals, formed chiefly of leucine, tyrosine and butalanine were deposited, and the mother-liquid, after dilution, complete removal of remaining baryta by sulphuric acid, filtration, and concentration, yielded several fresh crops of crystals. From the first of these the substance in question was isolated by repeated fractional crystallization. It is white, chalky-looking, crystallizing in larger or smaller balls. It is soluble in water, scarcely so in cold alcohol, and not at all in ether. When distilled, it fuses at 250°, and splits up into: first, water and the carbonate of a volatile base, these forming together in the receiver a thick liquid interspersed with crystalline plates; second, a white sublimate; third, a residue of yellow liquid which congeals on cooling. The distillate has a smell and taste reminding one of horse-radish, and fumes strongly on the approach of a glass rod dipped in hydrochloric acid. It yields not the least trace of tyrosin. Its analysis corresponds with the formula, $C_7H_{11}NO_3$, and when neutralized with hydrochloric acid it gives, with platinic chloride, an abundant yellow crystalline precipitate, having the composition $PtCl_4 . 2(ClH . C_8H_{11}N)$. The odor of horse-radish was found to be due to an oily base in the distillate, which base appeared to have the same composition as collidine, but will be the subject of further examination by the author. The sublimate has the characters of butalanine. The fusible residue in the retort is insoluble in water and in alcohol, and has the composition C_7H_9NO. According to these results, the pyrogenous reaction above described may be represented by the two equations:

$$C_7H_{11}NO_2 = H_2O + C_7H_9NO$$
$$2C_7H_{11}NO_2 = CO_2 + C_8H_{11}N + C_5H_{11}NO_2$$

The author proposes for the new amidated compound the name *tyroleucine*.

OSTRUTHIN.

By E. V. GORUP-BESANEZ.

OSTRUTHIN is a new crystalline body obtained by the author from the root of *imperatoria ostruthium* (masterwort). The young root is digested with warm alcohol, the extract slowly evaporated over a water-bath, and the brown residue extracted with a mixture of 3 parts of ether and 1 part of ligroin of low boiling point. To the resulting solution ligroin is added as long as a brown precipitate falls, and the clear liquid is left to crystallize by spontaneous evaporation. By repeated treatment in this manner ostruthin is obtained in the pure state. From its ethereal solution ostruthin crystallizes in dense white crystals belonging to the triclinic system. They melt at 115°, and resolidify at 91° to a yellowish waxy mass. Ostruthin is odorless, almost tasteless, and easily reduced to a white powder. On heating, it melts to a brown oil. It is insoluble in cold water, slightly soluble in hot water, benzene, and petroleum, and easily soluble in alcohol and ether. The alcoholic solution exhibits a faint sky blue fluorescence, which on the addition of a small quantity of water becomes a magnificent blue, surpassed only by aesculin.

Ostruthin does not contain nitrogen, and the mean of eight analyses gave carbon 77·07 per cent, hydrogen 7·98 per cent, corresponding with the formula $C_{14}H_{17}O_2$, which requires carbon 77·42 per cent, hydrogen 7·83 per cent. Ostruthin is dissolve by dilute potash and by aqueous ammonia, forming fluorescent solution. Its alcoholic solution gives no precipitates with alcoholic solutions of lead acetate, mercuric chloride, silver nitrate, or ferric chloride.—*Liebig's Annalen*.

TESTING AQUEOUS LIQUIDS FOR BLOOD.

By V. SCHWARTZ.

THE author made use of Teichmann's test, viz., production of hæmin crystals, the spectrum reaction, and the guiacum test, as carried out by Liman; he also determined the best precipitant for blood dissolved in aqueous liquids. In applying the hæmin test, the author allowed the acetic acid to evaporate spontaneously, he then obtained better results than by heating the slide before placing it under the microscope. The guiacum test was carried out exactly as described by Hünefeld : the author agrees with Leman in saying that if this test leads to negative results, we may conclude that blood is absent, but that positive results do not certainly indicate the presence of blood. Zinc acetate is generally the best precipitant for blood. One part of blood in 6,000 of water could be precipitated, and the hæmin crystals obtained: 1 in 15,000 could be detected by the spectroscope after precipitation. In water containing 17 per cent of common salt, blood could be detected by the hæmin test (after precipitation by means of zinc acetate), only when present in the proportion of 1 to 750. One part of blood was detected in 5,000 parts of soapy water by the spectroscopic and the hæmin tests, after precipitation with tannin solution. With zinc acetate, blood could only be detected when the proportion was 1 to 750 of soapy water. Small quantities of blood could not be satisfactorily detected by these tests when dissolved in urine.

One c.c. of blood was dissolved in 15,000 cb.c. of water (the water used was from a river). The blood could be detected, after precipitation by zinc acetate, by the hæmin or spectroscopic test after 8 days, but by the hæmin test only, after 10 days. The precipitate, with zinc acetate, was carefully washed with distilled water.

Small quantities of blood were detected in linen by treating with potassium iodide solution, precipitating with zinc acetate, and treating the precipitate with salt and acetic acid. This process may be applied to the detection of blood stains which have been already partially removed by washing and rubbing.—*Arch. Pharm.*

HYDROCARBONS.

By O. SILVESTRI.

IN the basaltic zone to the S.S.E. of Etna, at a distance of 22 kilometers from the central crater, there is a very ancient lava stream, a dolerite of dark brown color, almost basaltic in appearance, but which, when examined microscopically in thin section, is seen to consist of a crystalline base of augite, in which crystals of labradorite and peridote (olivine) are disseminated. In this lava there are cavities which, when broken open, appear filled with a liquid having the odor of mineral naphtha. Most of these cavities are small, but through Professor G. Pulvirenti the author was enabled to secure the contents of a comparatively large cavity 10 cubic centimeters in capacity, and also a supply of the lava from which this oily matter could be extracted by means of ether. The oil from the cavity had much the appearance of petroleum, being yellow by transmitted and green by reflected light. It was quite fluid at 24°, but at 17° it became almost solid from the deposition of crystals. The oil extracted by means of ether from the pulverized lava, which contained about 1 per cent of it, was very similar, but contained a larger proportion of sulphur than that from the cavity. The solid crystalline matter, separated from the oil by cooling it, was found to have the composition C_nH_{2n+2}; it melted at 57° and boiled above 300°; its behavior with solvents, and with various reagents, was precisely similar to that of the so-called paraffin, whether prepared artificially or obtained from a natural source. The oil expressed from the crystalline mass was submitted to fractional distillation, when the higher fractions by cooling, etc., gave a further portion of solid paraffin melting at 52°, making a total of 42·79 per cent. of paraffin in the original oil. Of the liquid hydrocarbons, a very small portion, 0·74 per cent, boiled at 79—88°; the rest at 200—400°; besides these, the oil contained 4·39 per cent of sulphur, and left 2·9 per cent of pitch on distillation. The author then discusses various hypotheses, as to the formation of this petroleum, which seems to have a special volcanic origin, contemporaneous with the eruption which produced the lava.—*Gazzetta chimica italiana.*

ORIGIN OF BACTERIA.

To the Editor of the Scientific American:

In the issue for July 21 of your valuable record of scientific discovery and progress, I find one of the ablest, calmest, and most impartial presentations of the spontaneous generation controversy now waging between Dr. Bastian and Prof. Tyndall, with Huxley and the brilliant M. Pasteur as coadjutors, which it has ever been my good fortune to peruse. But as your article, able as it is, deals only with the literature of experiment, perhaps you will permit an American microscopist to lay before your public the observations and experiments of several years devoted mainly to clinical investigation. In April last I repeated Bastian's experiments in the following manner. Having carefully tested two four-ounce bottles with absolute alcohol and placed in water of a temperature of 140 degrees Fahrenheit, I filled one of them with fresh urine at a temperature of 130 degrees, neutralizing with potassa of the same temperature; stoppered the bottle with a rubber stopper; dipped it to the shoulder in melted wax; drew a section of cotton batting over the whole, and tied it securely. Not a bubble of free air was left in the bottle. The second was filled with fresh urine passed directly from the bladder of a healthy man into the bottle; treated with potassa at 130, and stoppered exactly as the first had been. Two one-ounce test-tubes were then filled, the one with urine at 130, the other with fresh urine, treated with potassa, and left unstoppered in the rack. At the expiration of 13 hours a whitish cloud had formed at the bottom of each bottle and test-tube, and this cloud gradually involved the clear superincumbent liquid, until the contents of the bottles and tubes were of uniform color. Examined at the expiration of 36 hours, bottle No. 1 showed, by calculation, after counting 100 fields from as many different drops, bacteria to the number of 10,000 or more per drop; while in bottle No. 2 the development had attained the enormous figures of at least 1,250,000 per drop. The development of life in the open tubes was neither appreciably greater nor appreciably less than that presented in the bottles. In both instances the number of organisms in the boiled specimen was to that in the fresh specimen about as 1 to 125. Although such a calculation appears difficult to make, it is in reality a very simple and exact method of determination. With a $\frac{1}{14}$th inch objective, C eyepiece, draw-tube extended, the field of my microscope is almost exactly, by micrometer measurement, $\frac{1}{1000}$th of an inch in diameter. A thin cell of tin-foil, one half an inch in diameter, holding exactly one drop, furnishes, therefore, exactly 250,000 fields. That is to say, $\frac{1}{2} \times \frac{1}{2} \div \frac{1}{1000} \times \frac{1}{1000} = 250,000$. The average per field in the fresh urine from bottle No. 2 was a little in excess of five; the 100 fields counted gave in all 591 bacteria. In fresh urine from the tube, 523 organisms were counted in 100 fields. In the boiled specimens, seven bacteria were found in the bottled urine in counting 100 fields, and six in the specimen from the tube. These experiments were repeated three times with careful attention to detail, with substantially the same results that I have now described. The point is that while the influence of heat reduced the development of the bacteria per drop from 1,250,000 or more to about 10,000, the generative process was neither materially augmented nor materially diminished, neither hastened nor retarded, by contact with the external air. More recently, I have experimented with infusion of tea, with the result of less abundant bacteria, mixed with confervoid growths in great abundance. In an infusion of fresh grass, boiled four hours, and then bottled and sealed, bacteria, amœbæ and confervoid growths were abundant at the expiration of 120 hours.

I note these experiments very casually, because, as will presently appear, I am convinced that the spontaneous generation controversy is one to be determined by other methods—that, in a word, granting all that Dr Bastian claims, the problem is still unsolved, and one upon which the experimental generation of bacteria has no bearing. But recently, in company with Prof. D. C. Comstock, of this city, I examined a specimen of fresh urine from a young woman suffering from rheumatic arthritis, in which the development of bacteria was almost immeasurably greater than I have been able to produce experimentally; and this development, observe, took place within the living bladder. The exhibition of hyposulphite of soda had, at the expiration of 48 hours, so completely stopped their development in the bladder, that a specimen of urine showed innumerable dead, but no living organisms. The reaction was now acid. On neutralizing the specimen with potassa, it swarmed with life within an hour. Indeed, one of the strangest things about these creatures is the facility with which they die and come to life again. In another case, examined in company with the same gentleman, the patient having vesicular catarrh, with enlarged prostrate gland, a specimen of fresh urine was alive with vibrios. The reaction was neutral, oxalates and oxalurates abundant, with occasional crystals of cystin. I state these two cases as typical ones. In urine of neutral reaction, though normal in all important respects, bacteria are frequently abundant at the moment of passage, and presumably before; while an excess of oxalates and oxalurates as unerringly indicates vibrios. Again, the presence of confervoid growths in urine is an almost infallible indication of sugar, and, conversely, if I find sugar, experience has taught me to examine for the *penicillium glaucum*. I shall not be guilty of overstatement, I think, if I lay it down as a law of urinalysis, that there is a uniform relation between the neutral reaction and bacteria, oxalates and oxalurates and vibrios, and glucose and confervoid growths. Only yesterday I was called to examine a large blood clot just passed *per vaginam* with the products of a miscarriage, and on putting a particle of it under the microscope it was absolutely swarming with bacterial life, most assuredly developed *in utero*. The clot was however, not yet putrid, and the red corpuscles, though swollen and bluish, did not differ materially in appearance from those observed in venous blood. Obviously, the blood plasma only was concerned in this extraordinary development of alien life. I have, standing at my hand as I write, a bottle of innocent-looking Canada balsam. It is as perfectly clear as ever, and was obtained only yesterday from a druggist. But it is one mass of bacterial life, nevertheless, without presenting the slightest evidence of ferment, of putrescence, or of other alteration in physical constitution. June 17, having procured an appreciable quantity of dandruff from the head of a friend who suffers considerable annoyance from that source, I boiled it in a test-tube with a little water, added a drop of potassa, and decanted it; sealed the vial, and permitted it to remain unopened for 96 hours, at the expiration of which bacteria were abundant in the infusion. Take a drop of water from the gutter hours before any blue or other scum appears, and you will find it crowded with bacteria and confervoid growths.

Now, what is the origin of this vast and apparently spontaneous bacterial development that pervades the larger and visible life of nature? Is it propagated by genus produced by pre-existing bacteria, by genus generated in the atmosphere, or is its origin a spontaneous one? My own observations have led me to differ in opinion as decidedly with Bastian as with Tyndall. I have had bacteria in sealed cells under observation since April 13, and, although they are still living, I cannot find any evidence that they produce genus. A bacterium's term of life appears to be from 17 to 21 hours. As the old bacteria became inert, they gather into large heaps that intercept the light very considerably, and by and by from the margins of these heaps new bacteria emerge. Their reproduction is very slow, and the heaps have simply the appearance of inert granular matter—motionless, but striated as if the little creatures had arranged themselves in layers or rows, side by side, in such a manner that the organisms in each row are end to end with those in the next row, and so on. This is the general, but not the invariable rule. On the other hand, they sometimes form nearly circular masses that have the appearance of large cells—cancer cells, for instance—in the centre of each of which appears a minute nest-like depression. But the new bacteria, as before, constantly emerge from beneath the margin, and can not be traced inward by any glasses that I have been able to bring to bear upon them. Germs of a peculiar appearance are often observed in infusions where bacteria are present, but when their development is carefully traced, they will always be found to eventuate in confervoid growths.

The origin of bacteria in tissues, in excretions and in animal and vegetable infusions, is, I apprehend, a very simple process. They are not especially products of putrescence, as has been asserted in authoritative quarters, nor are they propagated by germs generated in the atmosphere, or produced by previous bacteria. How then? Experiments with urine, dandruff, saliva, mucous matter, and the pathological observations I have described, lead inexorably to a very simple view of that question. Substantially, that view may be stated as follows. The phenomena of nutrition, growth, and tissue decomposition in animal and vegetable bodies are accompanied with the disengagement, every instant of their lives, of millions upon millions of inconceivably minute particles of living protoplasm, each capable of a separate life. Every breath that you or I exhale is loaded, not only with a few blood corpuscles, epithelial cells, mucous granules, etc., but with intensely minute particles or molecules of living protoplasm, which, under favorable conditions, may grow and develop into separate microscopic organisms. The inference is irresistible, from our urine experiments, and from the case of the young woman I have mentioned, that the source of the development existed in each instance in the urine before it had any contact with the external air. By respiring against a slide, furnished with a glass cell, until I had gathered a drop or two of vapor, then hermetically sealing it, I have been able, in every instance in which I have tried the experiment, to produce bacteria from the products of respiration within 48 hours. It is impossible to describe processes minutely in a brief communication; but I will state this one fact as the result of a protracted series of such inquiries, namely, that the origin of bacteria is not to be sought in genus within the meaning of the term as employed by Prof. Tyndall, but in the growth and development, under favorable conditions, of particles of protoplasm disengaged from larger animal and vegetable bodies by the processes of respiration, perspiration, excretion, etc. A man, a tree, a rose, disengages millions of such particles during every instant of the vital term. To what extent, therefore, the world's atmosphere—and particularly the atmosphere of a large city—must be loaded with such particles it is impossible to form an adequate conception. If by genus Mr. Tyndall means protoplasmic molecules disengaged by the tissue-changes of larger organisms, then I am in agreement with him; but such, I apprehend, is not his view of the case. On the other hand, in this aspect of the subject, there is no ground whatever for the position held by Bastian, that the bacterial development in urine is produced by the recombination of organic bases under the influence of potassa, or that the problem is in any manner a molecular one. The influence of the potassa, by correcting the acid excess, furnishes merely a favorable condition for the growth of the myriads of protoplasmic particles disengaged from the blood plasma by the kidneys, and so excreted along with water, uric acid, salt, and other constituents. So far, then, as the bearing of bacterial life upon the question of spontaneous generation is concerned, Bastian's view is estopped by the fact that bacteria are, as exact observations have demonstrated, the secondary products of larger animal and vegetable life—not protozoa in any proper meaning of that term, but derivative lives always. If I seem to be bold in this assertion, and at variance with such masters as Tyndall, Huxley, Pasteur, Bastian, Haeckel, Cohn, and others; it is not from any wish to be eccentric or to offer a new theory, but the observations of years devoted to microscopic work have gradually driven me to adopt the simpler view of the subject that I have tried to elucidate in the foregoing paragraphs.

FRANCIS GERRY FAIRFIELD.

145 East Twenty-seventh Street,
New York, July 20, 1877.

THE CELL-DOCTRINE IN THE LIGHT OF RECENT INVESTIGATIONS.

By C. HEITZMANN, M. D., New York.

THE subject of my discourse is living or organized matter, that substance which builds up plants as well as animals, the simplest infusorium as well as the most highly developed mammal. The facts which I am about to explain are destined to throw light on many physiological and morbid processes, to assist the physician in the diagnosis and prognosis of diseases, and perhaps even to direct his therapeutical action.

The question, what living matter really is, cannot yet be answered from a chemical standpoint, and there is reason to doubt whether it ever will be settled, inasmuch as it is impossible to obtain pure living matter in a quantity sufficient for chemical analysis. In every substance the living matter must necessarily be composed of minute particles, which can never be seen, even with the highest magnifying powers, *i.e.*, the simplest units, the so-called molecules, which admit of no further division. After Elsberg's at present almost universally adopted denomination, we shall term the molecules of the living matter "plastidules." Molecules, again, are composed of simple elementary atoms, the quantity and nature of which give the essential character to every substance. While the molecules of inorganic bodies are formed by relatively few atoms—water-molecules, for instance, being built up of one atom of oxygen and one atom of hydrogen—we know that the plastidules are much more complicated in their atomistic construction. Every plastidule is formed by at least five elements, namely: 52 to 55 per cent. carbon, 21 to 23 per cent. oxygen, 15 to 17 per cent. nitrogen, 6 to 7 per cent. hydrogen, and 1 to 2 per cent. sulphur. The nature of the union of these elements is a very complicated one in every plastidule, but not as yet elucidated. We generally call the organic substances simply proteinates and albuminates—comprehending by these terms both the living matter and its derivations or products.

While chemical examination has revealed very little of the intimate nature of living matter, we know certain properties to be essential to living matter as long as it is really alive, and we know also some of its morphological features.

These properties are mainly two: motion, and the capability of producing its own kind. In speaking of the motion of living matter, we do not mean the motion to which every substance is subject, and of which light, heat, electricity, etc., are peculiar manifestations; but there are certain forms of motion dependent on the contractility or irritability of living matter which do not occur in inorganic bodies, nor in organic matter after it has ceased to be alive. The motion is of two varieties: one leading to changes of shape, the other to changes of place—locomotion. Both kinds are due to a peculiar structure of the living matter in a certain stage of its development, and will occupy us afterward. Here I will only mention that, in former times, locomotion was considered as a characteristic quality of animals. To-day we are fully aware that a great many of the low forms of vegetable life in different stages of development are endowed with locomotion, apparently depending on a certain degree of individual will.

The property of producing its own kind is exclusively possessed by living matter, and is also of two varieties, viz.: production for the benefit of the individual itself, with the result of increase size—growth; and production of new individuals—generation. We know that every living body is originally very small; the ovum of the largest animal is just perceptible to the naked eye, but it increases by taking up nourishing material from without—it grows. After having reached a certain size it does not grow larger, but only reproduces the used-up material, until at last it ceases to produce anything, and then becomes what we term dead, and thereupon is subject to chemical laws of decomposition, which means simplification of its atomic construction. To-day scientists have arrived at the conviction that the building material of plants cannot be essentially different from that of animals. With advancing knowledge of natural philosophy, the boundaries between the animal and vegetable kingdoms have more and more faded away. It is impossible, in many cases, to say exactly at what point of development an organism is certainly a plant or an animal. Huxley is of the opinion that the only distinguishing character between plants and animals is that the former feed on simple or elementary inorganic material, while the latter take in organized food; but this opinion can hardly be maintained, inasmuch as it is impossible to say how the lowest forms of animals are nourished at all. We know, moreover, through Charles Darwin's researches, that there are carnivorous plants.

The property of generation may be looked upon, in accordance with E. Haeckel's definition, as a growth of the individual beyond its individual limits; at least every organism must reach a certain degree of development before it is fit for propagation. This property is so characteristic for living matter that Lady Montague, having been taught by a philosopher that every organism is a mechanical apparatus, raised the question whether he ever saw, when putting two watches together, a third one resulting from it. It is known that among the lowest forms of organisms propagation takes place without sexual intercourse, whereas there is a division of labor among the higher organisms, both vegetable and animal: in the former case, one individual gives rise to a new one; in the latter, two individuals (male and female) are required to produce a third. It is known, furthermore, that the simplest form of propagation is division, when one individual, after having increased in size, splits into two organisms of smaller size. A variety of this process is the "gemmation," *e. g.*, a small bud, growing from the surface of the mother-body, becomes gradually pedunculated, and at length separates by breaking of the pedicle, and forms a new individual. Another variety is the "endogenous formation," in which a lump originates and grows within the mother-body, and is freed afterward through bursting or active perforation of the mother. Essentially all these processes are the same, and the main form of propagation is always a division. Even in the most highly-developed mammals the embryo originally forms a part of the mother-body, and, after having grown, by internal gemmation or endogenous production, up to a certain size, separates from the vehicle, the womb, and represents a new individual.

There are certain peculiarities about generation which I beg leave briefly to explain. I mean the resemblance of the newly-formed body to the producing organisms, the parents. It is an easy matter to understand that both individuals will be alike in a case of simple division, because both formerly made one single body; but how shall we explain the remarkable fact that, in higher animals, the offspring so closely resembles the progenitors, though only very minute parts of these—the ovum and the spermatozoids—contributed to give rise to a new individual?

The opinion of E. Hering, of Prague, that organized matter is endowed universally with an "unconscious memory," a function upon which depends, besides the capacity of imagination, of thinking, of habit, also nutrition and propagation, is not an available one. I therefore take into consideration only three modern hypotheses, of Charles Darwin, Louis Elsberg, and Ernst Haeckel.

Charles Darmin promulgated in 1868 the "Provisional Hypothesis of Pangenesis," which consists essentially in the assumption that through all stages of development the living cells or units of the body throw off small granules, or "gemmules," which accumulate to form the sexual elements, and all the cells of the body, therefore, participate indirectly in the new formation of organisms.

In 1872 Elsberg published his theory of the "Regeneration or Preservation of Plastidules." He lays down the proposition that the germ of every living individual contains plastidules of all its ancestors; so that these are bodily regenerated in their offspring, simply because bodily particles are preserved directly from generation to generation.

In 1875 Haeckel announced the hypothesis of the "Perigenesis of the Plastidules," according to which, in opposition to the opinions of Darwin and Elsberg, no regeneration or preservation and transmission of plastidules take place, but only a transmission of motion through inheritance.

Among these theories I confess that that of Elsberg seems to me the most probable one, inasmuch as it tries to explain why certain properties of ancestors, even in the second or third generation, may reappear; why bodily and mental peculiarities are directly transmitted from parents and grand-

parents to their offspring. With this theory, which suggests a direct increase of plastidules within a limited bulk of living matter, we may readily understand why, with progressive development of a species, a perfection takes place which leads to the production of more and more advanced beings from relatively lower ancestors. Haeckel's view can scarcely be supported so long as we know that a change of motion as function is always due to a material cause, namely, change of molecules in quality and quantity. All this is speculation only, though entirely legitimate as an attempt to bridge over precipices which present insurmountable obstacles to the passage of our intellect.

Let us advance now toward the study of the shape of living matter, a study in which excellent investigators have been engaged during the last forty years.

In 1835 Dujardin discovered a contractile substance common to low animals, which he termed "sarcode," but he was far from the knowledge that this substance exists in all animals, believing it to be peculiar to the lowest forms. After Schleiden, of Jena, in 1838 discovered the form-elements of plants, and proposed for them the name of "cells," Theodor Schwann, of Berlin, in 1839 found a striking analogy between the intimate structure of vegetable and animal organisms, and asserted that the "cells" are the simplest constituent parts of all tissues of the animal body as well as of the plant. In his opinion each cell is a vesicle composed of a transparent membrane, containing a fluid in which is suspended a central solid body, the nucleus. Schwann believed that cells may originate in a substance, the plasma, independently of former cells, and through the authority of Johannes Müller, of Berlin, who fully accepted Schwann's doctrine, this became the leading one, so that even C. Rokitansky, of Vienna, held at first that the plasma of the blood may, under favorable circumstances, produce cells. It was the discovery of Rudolph Virchow, in 1852, that the cells are really the seats of life, and that every cell must originate from a former cell: *Omnis cellula e cellula*. Virchow, however, still adheres to Schwann's original idea as to the construction of cells, although a very simple consideration will show that this cannot be correct, viz., the consideration of the fact that no living material is ever a fluid, but always either a solid or a jelly-like, semifluid substance. The next who advanced the cell-doctrine was Max Schultze, of Bonn. He showed in 1861 that changes of form, locomotion, and division are impossible to corpuscles surrounded by a resistant membrane; he maintained that the smallest individual elements of organisms are lumps of a jelly-like matter endowed with life, for which he proposed, for good reasons, in accordance with the German botanist, Hugh von Mohl, the term "protoplasm." This jelly-like substance is identical with Dujardin's "sarcode." Max Schultze was the first to announce that the living matter of the infusion animalcules and that of the cells of all animals are one and the same substance. The cell consists, according to this observer's views, of a minute particle of protoplasm, in which there are imbedded the nucleus and granules. In the same year (1861) E. Brücke, of Vienna, though accepting Max Schultze's views, asserted that the nucleus is not an essential part of the cell, as he knew of many living lumps without any nucleus. Brücke defined the "cell," for which he also proposed the name of "elementary organism," to be a structureless lump of protoplasm; though fully aware of the necessity of the existence of some structure, as in every substance, he regarded the structure of the cell as imperceptible to our senses. S. Stricker, in accordance with Brücke, in 1868 explained that the cell is nothing but a particle of structureless protoplasm, usually containing granules, but that these granules are not essential characteristics. He especially examined the form-elements of the ovula of frogs while studying their development, and observed in these elements hyaline flaps, which he took for pure protoplasm, whereas the greater part of the protoplasm was filled with granules or particles of yolk. The fact that every living lump is capable of taking foreign minute corpuscles, granules of carmine or aniline for instance, from without, led him to the conclusion that protoplasm is devoid of any visible structure, while the visible granules are secondary products of the protoplasm or foreign substances accidentally taken into the interior of the protoplasmic lump. S. Stricker in his "Histology" discusses the question, "how large the lump of protoplasm must be to be entitled to the name of cell," and comes to the conclusion that we should call a living corpuscle a "cell" only when we perceive in it the properties of a living organism, viz., growth, motion, and reproduction. In 1862 Lionel Beale, of London, accepted Max Schultze's doctrine and the term "protoplasm," arriving, however, at conclusions quite different from those of German biologists. Apparently his microscopes, although magnifying very much, did not show him the details within the protoplasm, and thus, judging especially from carmine staining, he asserted the nucleus to be living or "germinal" matter, while a great deal of protoplasm was to him identical with the basis substance of connective tissue, and he termed this the "formed material," designating by this term even the most active tissues, the muscles and nerves.

This was the state of the cell-doctrine when I took up its study in Vienna in 1872. After having observed certain peculiarities in various protoplasmic bodies, I endeavored to investigate the condition of living matter in so-called infusions, and I may say that in this I was successful.

HOW TO HATCH INFUSORIA.

If we place some black earth and green blades of grass in some Croton water, in a soup-plate, and allow this infusion to stand undisturbed in a light room at ordinary temperature, we shall always succeed in raising new organisms, so-called infusoria. There are different ways of making an infusion: for instance, we may simply infuse some hay or chicken's food in water; but I prefer the method mentioned, on account of the certainty of producing important low forms of animalcules.

Strange to say, but it is a fact, that if we mix together some inorganic material, earth and water, with organized bodies, such as grass, apparently destined to decay, there will sprout up a remarkably rich generation both of plants and animals. To explain this fact is quite difficult. Some observers believe that the decaying particles of vegetables themselves change into new organisms under favorable circumstances; while others, and doubtless the majority, are of the opinion that there are floating in the air millions of invisible germs of plants and animals, which, on finding a favorable soil for development, begin to grow and prosper. The germ-theory, first thoroughly established by Pasteur, has not as yet been contradicted in a satisfactory manner; we have, therefore, every reason still to adhere to it. Certainly no development of infusoria takes place if the air be prevented from reaching the infusion.

AMŒBÆ.

It is very remarkable that I succeeded in raising almost identical forms of living organisms on mixing together the same material several thousands miles away from New York, viz., Vienna. There is a slight difference, however, important enough to be mentioned. In Vienna I never saw an amœba without a distinct lump in its interior, the nucleus; while in New York the more common occurrences are amœbæ without nuclei. As these animalcules are identical in every other respect both in Vienna and New York, this fact disproves the opinion of many histologists that the nucleus is something essential to the so-called "unicellular" organism. Haeckel's view, viz., that there is a marked difference between forms devoid of a nucleus, termed by him "cytodes," and those with nuclei, termed "cells," must be considered to be untenable.

REVELATIONS OF THE MICROSCOPE.

Let us take from the infusion, best from the sides of the plate, a drop; place it on a slide, cover it with a thin covering-glass, and examine it carefully with a high magnifying power, of about one thousand, with a good immersion-lens. On the first or second day after making the infusion we do not succeed in discovering any organism in it, unless such were already present on the torn grass. About the third day we see a number of very minute granules, just perceptible to the highest powers of the microscope; these granules are yellowish, shining, and motionless. One day afterward, besides such small granules, there are seen very many somewhat larger ones, which in their interior show a central hole, inclosed on all sides by a yellowish, shining substance. That this is the case can be proved by adding a drop of water, whereupon the granules turn and always present the same aspect. The cavities in the interior of the shining lumps, apparently filled with some fluid, differ in their refracting power from the surrounding mass; they show a slight rosy color and bear the name of "vacuoles" in histology. A vacuole is the first sign of differentiation within the lump, though the latter is still immovable.

One day later we see, besides the forms already described, somewhat larger, round granules with several vacuoles, some granules looking as if perforated by vacuoles, like a sieve; the differentiation between the two substances within the lump—the yellowish, shining, and the colorless, rosy refracting one—has apparently advanced. On the fourth or fifth day we have before us a certain number of small living plants and animalcules. Among these let us choose a lump, which, looked at with magnifying power of about five hundred, has already shown us wonderful changes of shape after a few minutes' rest, due perhaps to the shock of the transport from the plate to the slide. Such a minute transparent corpuscle, floating in the water of the infusion, constantly changes its outlines, by throwing out offshoots or processes, mostly in the form of hyaline flaps. We are sure we have an amœba before us.

LIFE AND MOVEMENT OF AMŒBÆ.

The best species for our examination is the common *Amœba diffluens*, which slowly moves in one direction by protrusion of single flaps; the star-like and giant amœbæ are less fit for close examination, both on account of their rapid and complicated changes and their limited viability. With a magnifying power of five hundred we recognize in the nucleated amœba a shining lump, viz., the nucleus; around this a small, light seam, not uniform during the motion of the lump; and minute grayish or yellowish granules, scattered throughout the transparent mass. The floating amœba throws out offshoots in all directions, and retracts them again; it therefore changes its shape, but does not move away. Presently we observe, by careful handling of the screw of the microscope, that one of the processes reaches the surface of the covering-glass or of the slide, and at the next moment the floating ceases and creeping begins. The amœba protrudes, on one side of its body, a hyaline flap, while on the opposite side an apparent accumulation of the granules takes place; shortly afterward the granules are again uniformly distributed, and the whole lump is dragged toward the point of the protruded hyaline flap.

Let us now adjust the immersion-lens. We recognize in the center of the body the apparently solid or vacuolized, roundish nucleus, and this surrounded by a narrow, transparent seam. The latter is traversed by very delicate, slender, conical, grayish threads, of which the thicker ends emanate from the nucleus, and the points are attached to the nearest granules, scattered in the body of the amœba. Many of these granules are connected with each other by slender threads, and are thus in direct communication with the denser stratum circumscribing the amœba. In this way a delicate network is formed, with nodules, represented by the nucleus and the granules. Whenever a hyaline flap is being protruded, there appears on the opposite side of the body an approximation of the granules to each other, and a perceptible enlargement of the same; in the protruded flap itself the granules, on the contrary, become smaller and separate from each other until all structure disappears from our view, and the flap represents a homogeneous mass. After having reached a certain size the flap again presents the above-mentioned structure, and the granules float toward the protruded point, and drag with them the nucleus, which itself never shows active change of shape or motion. The locomotion of the amœba is thus accomplished. At the next moment the same spectacle is seen again and again, perhaps for an hour, until the movements become slower, and at last we have before us an immovable round body, when the amœba is dead.

During the movements of the amœba vacuoles not unfrequently make their appearance within the body. Every vacuole appears to be lined by a thin, shining, continuous layer, analogous to the layer which surrounds the whole body. Sometimes floating granules can be observed within the vacuole, which, by turning around, change their places and now and then throw out delicate threads; occasionally some of these threads reach the wall of the vacuole, then the latter suddenly disappears and the network is re-established. Such changes in vacuoles are frequently observed in the body of another infusion-animalcule, the shooting cyclidium, and also of human colorless blood-corpuscles.

By adding a drop of distilled water to the preparation we succeed in making the amœba swell up, whereupon most of the granules, apparently torn apart, jump about very actively in the interior of the body. By adding a drop of glycerine, on the contrary, the amœba is suddenly contracted, and forms a small, almost homogeneous lump. If we substitute water for the glycerine the amœba always changes into a granular globule, in which the original structure is again plainly visible, though the globule is motionless and apparently dead.

THE FIRST FORM OF LIVING MATTER.

The changes in the infusion just described allow the conclusion that living matter appears at first in the form of a small granule only; gradually through differentiation becomes a vacuolized body, in which it forms a framework, and is at last arranged as a network; the latter state being that of a common protoplasm. As one and the same granule cannot be followed up in its gradual development in the infusion, we must look for other living corpuscles, to see these changes directly. Such corpuscles are those suspended in the blood of the common fresh-water crawfish (*Astacus fluviat.*).

For examination of the blood, we prepare the covering-glass by greasing its edges on one surface with olive-oil. If we break off the leg of the living animal, fresh from the river, a drop of the colorless blood will ooze out, which being caught on the slide, and covered with the oiled covering-glass, remains unchanged, as the evaporation of the plasma is prevented. No other preparation than that for producing a so-called "moist chamber" in the simple way mentioned is required for the examination of the blood of cold-blooded animals.

Immediately after placing the blood upon the slide most of the blood-corpuscles look coarsely granular, viz., are filled with shining, yellowish, round granules. Shortly afterward every granule begins to enlarge and flatten; so much so, that by mutual accommodation the granules become polyhedral, being separated from each other by small hyaline seams, which are traversed by very distinct, grayish, intersecting threads. Soon every flattened granule is provided with one or two vacuoles; the yellowish substance surrounding the vacuoles suddenly commingles with the same substance of neighboring granules, and, as if bursting, is transformed into a delicate, finely-granular network, in the midst of which a formerly invisible nucleus can be seen. The nucleus remains immovable, while the body, now pale and finely granular, still changes its shape.

In the blood-corpuscles of crawfish, therefore, the changes of the granules, from originally solid, homogeneous bodies into vacuolized ones, and at last into a delicate network, can be seen directly in the course of about half an hour. The body resulting from the changes of the granules of the blood-corpuscle is analogous in every respect to the amœba.

Colorless blood-corpuscles of the newt (*Triton, Salamandra*) and the frog can also be used for the study of the structure of the protoplasm with a very satisfactory result. By cutting off with scissors the point of the tail of a newt, or a toe of a frog, we are enabled to transport a drop of blood directly to the slide, and by covering the drop with a thin glass, oiled on its edges, we obtain admirable specimens for examination even with the highest powers. The finely-granular, colorless blood-corpuscles of the newt show, at the common temperature, continuous changes of shape and place by projecting a great number of delicate offshoots in all directions, and again retracting them. During this time the grayish or yellowish network in the interior of the body is also constantly changing in shape. Very often vacuoles appear in the body, and sometimes the body looks as if perforated by vacuoles, like a sieve. In such a body the identity of the framework surrounding the vacuoles, and of the network inclosing hyaline spaces of varying size, can easily be proved; at all events, the body reveals a sponge-like structure. Excellent subjects for watching the structure with high powers are also the colorless blood-corpuscles of frogs.

THE BLOOD-CORPUSCLES AND ALL THE ELEMENTS OF THE BODY COMPOSED OF AMŒBÆ.

Blood-corpuscles of mammals and of man must be examined on a heated stage, in order to raise the temperature of the specimen to that of the body. The specimen is easily obtained by pricking the palmar surface of the hand. At the common temperature of the room no structure is recognizable in my own colorless blood-corpuscles, but the structure becomes the plainer the more the temperature of the stage approaches the normal heat of the body. There appear one or two grayish, homogeneous lumps in the interior of the corpuscle, and from the periphery of the lumps many conical, slender, grayish threads emanate, which unite with other threads, and form at their points of intersection somewhat thickened nodules, or granules, the whole constituting a complete, grayish network throughout the corpuscle. A continuous grayish layer circumscribes the periphery of the corpuscle, in close connection with the most external threads. While the temperature rises slowly, continuous changes in the shape of the network and of the whole corpuscle take place; but locomotion occurs only if the specimen has been inclosed between two thin glasses, which furnish the corpuscles with points of fixation. Distinct nuclei, with nucleoli, all united by means of grayish threads, appear only when the motion of the corpuscles becomes slow, or when the corpuscles approach the state of rest, viz., that of death.

In colostrum-corpuscles, present in milk during the first days after delivery, the network is also plainly visible; many of the granules are here changed into shining fat-globules, but are still in connection with their unaltered neighbors. Later on, all the granules of the colostrum-corpuscles appear to be transformed into fat-globules.

The blood-corpuscles of the crab, and especially those of the oyster, are also excellent for studying the structure of protoplasm. If we break the shell of any oyster and cut the animal, we may bring its colorless blood under the microscope and watch the structure of the blood-corpuscles, and their striking changes of shape, and even their locomotion. We always see before us organisms entirely identical with different forms of amœba.

Lastly, I may mention that products of inflammation, pus-corpuscles, as long as they are alive, show exactly the same structure and capability of creeping as are visible in an amœba; the latter fact having been discovered about ten years ago by Von Recklinghausen, of Strasburg. All the elements of the body, including those of the tissues, agree with the amœba in respect to their structure and viability.

Before advancing in my discourse I wish to say a word in regard to the correctness of my assertions, inasmuch as observations of so delicate a nature, with high magnifying powers, have always been liable to error.

There are two ways of satisfying one's self of the correctness of a microscopical interpretation: demonstration to others who are unprejudiced and impartial observers, and representation independent of our senses, viz., by photographs. Both means have been adopted with the discovery of the structure of protoplasm. Dr. L. Elsberg, who spent several months in examining the protoplasmic bodies in my laboratory, arrived at the conviction that my views are correct, and announced this conviction in the "Transactions of the American Medical Association" in 1875. Dr. Elsberg is of opinion, in which I agree with him, that no preliminary studies in microscopical science are required for seeing the intimate structure of the living elements. An unprejudiced observer, with a practice of a few days, will be able to satisfy himself in regard to matters which have been overlooked by many experienced microscopists, apparently be-

cause of their prejudices or belief in asse[illegible] authorities. Besides Dr. Elsberg, many gentlemen, and also a lady, have satisfied themselves of the truth of the structure of protoplasm during the courses of instruction in histology which I have given in my laboratory for two years. Dr. J. J. Woodward, of Washington, has published beautiful photographic plates of cancer-specimens, made with oxycalcium-light, with relatively low powers, and on these plates the structure of the protoplasm is plainly visible to the naked eye, where ever the elements lie in a correct focus. The objection that this structure might perhaps be due to coagulation, to changes after death, falls to the ground, as on looking at photographs of living blood-corpuscles, such as have been produced by Dr. Cutter, of Boston, the network is as plainly visible as in the plates of Dr. Woodward. Doubtless the photographs of living and creeping amœbæ would give the same result.

Those, however, who are delighted with nice staining of microscopic specimens, splendid projections on screens, and large micro-photographs, generally lose sight of the aim of the science of microscopy. We have other things to do than to play with methods of staining and projections. We have to study the relations of physiological and morbid appearances to their anatomical bases—a more serious and difficult task. Photographing microscopical specimens has reached its highest perfection in this country, where technical talent is so remarkably developed. Although such photographs are useful in certain respects their value should not be over-estimated, because they are indistinct wherever the specimen is not even or shows several strata. Under such circumstances photographs can hardly replace drawings made by an experienced and conscientious artist.

STRUCTURE OF PROTOPLASM.

Let us proceed now to the consideration of the structure of protoplasm as explained schematically. Judging from the observations in an infusion, or in blood-corpuscles of crawfish, it is plain that living organisms originally form homogeneous lumps. These, with advancing growth, are differentiated by the formation of vacuoles into a framework, and finally into a network. In both latter states we distinguish two substances, one identical in every respect with the substance forming the first homogeneous lump, the other contained in the vacuoles and in the meshes of the fully-developed protoplasmic body. The substance forming the framework and afterward the network is endowed with the properties of contractility, and—as it originated from a small lump—growth; therefore it is living matter. On the other hand, the substance contained in vacuoles, and in the meshes of living matter, never presents signs of life, being a fluid. That it is not pure water is prove by the phenomena of diffusion on adding a drop of water to the specimen. The solid nucleolus, the solid nucleus, the granules, their uniting threads, and the surrounding layer of the whole body, are formations of living, contractile matter, which are suspended in a non-living fluid; *i. e.*, the network contains in its meshes and incloses as a shell on the surface of the corpuscles, and also a hollow nucleus, a non-contractile fluid. The fully-developed protoplasmic body, therefore, is constructed like a sponge, but, at the same time, inclosed on all sides by the same substance which forms the trabeculæ of the sponge—the trabeculæ and the shell being the living matter.

An analysis of the observations of the living protoplasmic body teaches us that there can be distinguished mainly three different appearances of the net-like living matter, namely, that of rest, that of active contraction, and that of passive extension.

In the state of rest, as seen in a motionless amœba, or immediately after death, the granules are almost uniformly distributed throughout the protoplasm, united with each other by slender threads, the bridges of living matter. (See Fig. 1.)

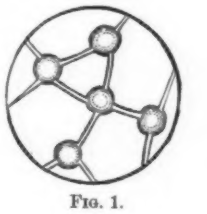


FIG. 1.

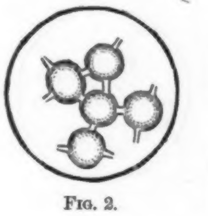


FIG. 2.

In contraction we observe an enlargement of the granules by shortening of their uniting threads and approximation to each other. (See Fig. 2.) Nothing has been added to the living matter and nothing lost from it; only the distribution of the plastidules has changed, leading to the narrowing of the network and a partial expulsion of the fluid formerly contained in its meshes. Contraction is the active property of living matter, and on it are based the simple change of shape and the locomotion of the whole organism.

Extension depends upon a decrease of size of the granules, with a removal from each other and an elongation of the uniting threads at the expense of the bulk of the granules, even to the disappearance of all structure. (See Fig. 3.)

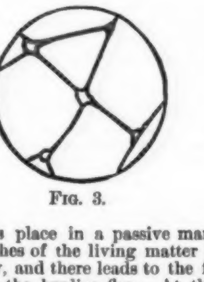


FIG. 3.

The extension takes place in a passive manner; the fluid contained in the meshes of the living matter is pushed out toward the periphery, and there leads to the formation of a protruding offshoot—the hyaline flap. At the beginning of the protrusion we still observe in the flap the presence of structure, while at the highest point of extension the structure can no longer be seen, because granules and threads have been elongated to their utmost capability. We may compare this phenomenon to the extension of glass rods melted in a flame until the threads become so thin as to disappear to the naked eye.

These three states of living matter explain to us not only the movement of a simple protoplasmic lump, but also the action of the most highly-developed muscles, which, as I have demonstrated, are entirely identical in their structure with the simple amœba. Were the amœba a sponge without an inclosing layer of living matter on its surface, every contraction would lead to an escape of the fluid, and no locomotion would be possible; the presence of an outer, although very thin, layer of living matter is necessary to the various movements of living protoplasmic bodies.

By adding a drop of glycerine to the creeping amœba, or to any protoplasmic body, we can bring about a fourth state of living matter, viz., the highest degree of contraction, for which S. Stricker and I have proposed the term "tetanus." The fluid of the protoplasm being suddenly extracted by the glycerine, all granules flow together, forming a structureless lump of much smaller size than that of the original corpuscle, without visible limits of the single granules. (See Fig. 4.) As mentioned before, a rehabilitation of the former net-like structure is produced by taking away the glycerine and adding water, without re-establishment of motion.

All these changes of living matter can be directly seen under the microscope. But we cannot observe the formation of a flat, extended layer at the boundaries of the whole

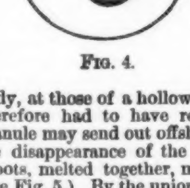


FIG. 4.

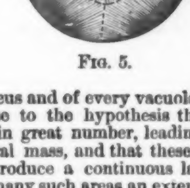


FIG. 5.

body, at those of a hollow nucleus and of every vacuole. I therefore had to have recourse to the hypothesis that a granule may send out offshoots in great number, leading to the disappearance of the central mass, and that these offshoots, melted together, may produce a continuous layer. (See Fig. 5.) By the union of many such areas an extensive layer could be produced, large enough to cover in the whole protoplasmic body.

The presence of a layer of living substance on the outer surface of the body explains to us why every protoplasmic lump can so easily take up foreign bodies, and why vacuoles can form and disappear almost suddenly. We must imagine that the living matter is capable of entering any of the described states at any time, so that a flat layer, for instance, may immediately change into a network, and *vice versa*. When the lump swells up through the addition of water the granules are torn apart and float freely in the fluid, as occurs in swelled amœbæ and saliva-corpuscles. The breaking of the outer shell, with escape of minute particles of the amœba, still endowed with life, and the process of the division, can also easily be understood.

Let us now draw conclusions from the observations described.

Protoplasm is not structureless, but has a very distinct net-like structure. The protoplasm, forming a so-called "cell," possesses a very complicated structure, and therefore the cell, in the sense of M. Schultze and E. Brücke, is not an elementary organism. As stated above, every particle or granule of living matter, very many of which go to build up a protoplasmic lump, is endowed with all the properties of the so called "cell"—growth and motion. The question is, How large must be a simple particle of living matter to entitle it to the name of an individual organism?

GROWTH AND DIVISION OF GRANULES.

In the infusion we see growing granules, just perceptible to the highest magnifying powers of the microscope, in a fluid where there were none a short time before. S. Stricker, when examining the renowned Lostorfor's corpuscles, which for a short period of time were thought to be characteristic of syphilis, made observations analogous to those which I have described. He saw extremely small granules originate in the plasma of the blood of persons broken down by different diseases; the granules, which were first just perceptible, grew under the eyes of the observer, and, after having reached a certain size, divided into two parts, producing new individuals. Moreover, we know of minute organisms in decomposing organic tissues—the micrococci—which are just on the limit of perceptibility, and notwithstanding endowed with motion and growth. It is plain, therefore, that the size of a living body is irrelevant in the definition of an organized individual. The smallest which we are capable of seeing with the best microscopes of to-day are granules; but we must admit that germs or particles of living matter may be present in the air or in organic fluids in infinite numbers which cannot be seen at all, and become visible only after having reached a certain size. How complicated the structure of a minute particle of living matter may be we can hardly imagine.

The term "cell," as proposed by Schleiden and Schwann, had long been considered as a misnomer, since M. Schultze demonstrated the absence of a lining membrane and fluid contents. I have demonstrated that the so-called "cell" is not an elementary organism, but that it is composed of innumerable particles of living matter, every one of which is endowed with properties formerly attributed to the cell organism.

According to my observations, we have not to deal with "cells" as form-elements, either in the fluids or in the tissues of the animal body, but only with living matter, varying in its appearance from the just-perceptible granule to the bulk of the body of the largest animal itself. Single lumps of living matter may show the net-like arrangement, being then termed "protoplasm;" while the body of a mammal is a continuous mass of living matter in net-like arrangement and contains fluids, in which there are suspended isolated bodies, analogous to the granules which float in the vacuoles of an amœba. The difference in the aspect of the tissues depends on the presence of a lifeless basis-substance only, a derivative of the lifeless protoplasmic fluid, while essentially all tissues are formed by protoplasm, in which living matter without interruption is united throughout the body. I may add that, according to my recent investigations, plants are built up in a way exactly corresponding to animals.

Let us give up the term "cell," as now no proper sense is connected with it; let us give the usual name to every organism, amœba, for instance, or say blood-corpuscles, pus corpuscle, cartilage-corpuscle, etc., if we intend to designate separate lumps of protoplasm, or certain constituent parts of different tissues.

VARIABLE QUANTITY OF LIVING MATTER IN DIFFERENT INDIVIDUALS.

In conclusion, I may draw attention to the fact that the amount of living matter varies greatly within a limited bulk of protoplasm, both in normal and morbid conditions. The colorless blood-corpuscles of persons exhibiting signs of lymphatic, strumous, scrofulous constitutions contain much less living matter than those of strong, vigorous persons. Further examinations will in all probability teach us to make use of these differences for practical purposes. I announced three years ago that the protoplasmic lumps forming tubercle are characterized by a relatively small amount of living matter. Last year I published my observations on pus-corpuscles, which enable me, from the relative amount of living matter contained in an individual corpuscle, to say from what kind of organism such a pus-corpuscle is formed; whether the person from whom the pus comes is healthy and strong, or weakened by chronic disease, as tuberculosis.

I am thoroughly convinced that the doctrine here explained, for which Dr. Elsberg proposed the term "bioplasson-doctrine," must eventually be accepted by all judicious microscopists. The popularization of even the simplest facts takes time. Though at present objections to my observations may be raised, their correctness can never be shaken. The truth will finally triumph.—*New York Medical Journal.*

THE GENESIS OF NERVES.

THE following interesting extract is from a lecture delivered in London by Mr. G. I. Romanes:

If the swimming bell of Aurelia be cut and so unrolled that, roughly speaking, it forms a parallelogram, and all the ganglia be removed except one at one end of the parallelogram, then if a gentle stimulation be given at the other end, too gentle in itself to start a contractile wave from the point stimulated, there will, nevertheless, in a little while be a contractile wave started from the other end, from the ganglion, thus showing that a stimulus wave must have passed through the contractile sheet to the ganglion, and so caused it to discharge. In some cases the passage of this stimulus wave admits of being traced. The numberless delicate tentacles which fringe the margin of this medusa are more excitable than is the contractile tissue of the bell; so that a stimulus which is not strong enough to start a contractile wave in the bell may start a contractile wave in the tentacles, one tentacle after another contracting in rapid succession, till the wave of stimulation has passed all the way round the disk. These facts prove in a beautiful manner that the tissue is already so far differentiated from primitive protoplasm that the distinguishing function of nerve has become fully established. And now this very important question arises: Does this conductile function prove itself as able to survive the process of severing as the contractile function has always been found to be? Mr. Romanes has found that it is as tolerant. It is quite as difficult to block the passage of stimulus waves by means of interposing cuts, as it is to block the passage of contractile waves by the same means. This is, perhaps, the most important observation, both to the physiologist and the evolutionist, that has ever been made in the whole range of invertebrate physiology. To the physiologist, it demonstrates that the distinguishing function of nerve, where it first appears upon the scene of life, is a function which admits of being performed vicariously to almost any extent by all parts of the same tissue mass. To the evolutionist, it demonstrates the existence of such a state of things as his theory of nerve genesis would lead him to expect. In the case of a medusa cut into a spiral strip, it was noticed that where the waves became suddenly blocked by section, in about 90 or 95 per cent. of cases such blocking was permanent; but in the remaining 5 or 10 per cent. of cases, after a time that varied from a few minutes to a day or more, the obstruction is overcome, and the contractile wave passes forward with a perfect freedom. This is not due to what physiologists call shock. The explanation of a temporary blocking is of great interest, and the following hypothesis is probably the true one. Suppose there is a well differentiated line severed by the cutting, and near it an uninjured line less differentiated, and which, while almost, is not quite able to convey the stimulus. The waves of contraction and stimulation are no longer able to pass along the usual line, now severed, and as they perpetually "break" upon the area of blocking, each of the forces concerned seeks for itself the lines of least resistance. The principal line will be the partly differentiated line, which is already nearly able to carry on the wave of stimulation. Every wave imposes a much higher degree of functional use on this line than it was ever before required to exercise, and as the greater use causes greater permeability, the line, from being almost, is soon quite able to carry a wave of stimulation, and so to set up a wave of contraction beyond the previous blocking. As might be expected, the first waves were feeble, but they were observed to get stronger and stronger, till at last, as the nerve passage became more permeable by use, they poured on, without any perceptible diminution of force. Mr. Romanes also described another species of medusa, which he has called *Tiaropsis indicans*, which has a more highly differentiated nervous "system;" and whose polypite turns to the direction of a stimulant.

RHINOPLASTY BY THE TALIACOTIAN PLAN.

THE subjoined interesting case was related to the Clinical Society of London, by Mr. MacCormac. The patient was a healthy girl, aged sixteen, who had lost the tip and both alæ of her nose, when an infant, from the injection of a nævus, situated on the nose, with the pharmacopœial solution of pernitrate of iron, which had resulted in a slough. Mr. Mac Cormac determined to attempt relief by means of Taliacozzi. The apparatus for keeping the arms in position consisted of a pair of ordinary stout, well-fitting stays, with two perinæal straps attached, to prevent displacement upward; a helmet, partly made of leather, connected to the stays by a leather band running up the center of the neck and back; an arm piece, strengthened by a steel band, moulded in leather, and extending from the wrist to the shoulder, where it was buckled to the stays; and a gauntlet for fastening the wrist and hand to the helmet, while the elbow could be fixed in any required position by straps running from it to the stays, and to the sides of the head-piece. There was nowhere any undue strain, and the apparatus was applied for days before the operation, to accustom the girl to its use, and remedy any defects in it. It proved to be so comfortable that she could sleep in it. A model of the part required to make good the deficiency was made in gutta-percha, and from this the extent of the deficiency could be projected on a flat surface. On February 12th, 1877, a flap was marked out on the inner aspect of the left arm, two thirds larger than the actual size of the estimated deficiency; it remained attached to the arm by a broad long pedicle, and consisted of all the tissues down to the muscular sheath. A triangular gap was next made on the right side of the nose, and into this the flap from the arm was fitted, where it became partly

united by the first intention. Subsequent suppuration occurred around the flap, but it had all united in three weeks. Then the operation was completed by detaching the base of the flap from the arm, and preparing the left side of the nose to receive it, where it was adjusted by sutures. In a fortnight healing was completed. The dressings of the arm and nose consisted of cotton wool soaked in olive oil. The leathern apparatus answered its purpose completely and without inconvenience. For the first two or three weeks after the operation, much contraction of the new nose occurred, but very little subsequently. The extensive wound in the arm had nearly healed on May 11th, 1877. The nostrils were kept dilated by india rubber tubing. The improvement in the girl effected by the operation was most satisfactory.

DISEASE SPREAD BY TAILORS.

A DELEGATION from the Amalgamated Society of Tailors recently waited upon the British Government in the person of Under Secretary Cross. Their object was to lay before him some facts in connection with what was called the "sweating system." One of the delegation said he had seen instances in which garments were lying on a bed in which fever patients were suffering. There were a great many instances in which such things had taken place. They considered that if an employer got people to take work home, he should be bound to get the place to which it was taken registered, and hoped Mr. Cross could see his way clear to make it imperative that every shop used as a tailor's workshop should be so registered by the employer. A delegate from Manchester gave the results of visits to 1,000 homes where this work was carried on, and stated that the condition of things was something deplorable. In some cases four or five persons were at work in a room nine by twelve feet. Sometimes people were making these garments in the midst of their domestic arrangements. From the facts that had come under his knowledge, he had no hesitation in saying that the state of things required alteration, and that the people engaged were in a most unhealthy condition. They found somewhere near 1,300 people engaged in this way, and all the surroundings of the place were such as would foster and spread disease. Another delegate said in some instances in London a man and woman would be at work in a small room at the top of a house, in which they lived and slept. The people occupied in this were so crowded together that the place could not fail to foster and spread disease. While people went to large shops with showy fronts, they did not know that the clothes they purchased were made in close and unhealthy rooms. He knew of a case in which, while the body of a child who had died from small-pox lay dead on the table, and two children were sick with the disease, the man and wife were at work in the same room, and twelve fashionable coats were in the room, which would be sent all over the town.

A NEW METHOD OF CURING POPLITEAL ANEURISMS.

DR. MARTIN BURKE, of Bellevue Hospital, reports three cases of popliteal aneurism, that were cured by compression of the femoral artery by means of a conical bag filled with shot, which was suspended from a height in such a way that the apex of the cone pressed on the artery in Scarpa's triangle. In the first case pulsation in the aneurism ceased in eight days; in the second, in sixteen days; and in the third, in six days. The cure was slow in the second case, on account of the patient's neglect to keep the apparatus in place. During the treatment little or no pain or uneasiness was complained of in any of the cases.

The shot bag was made of canvas, in the form of a flattened cone, the apex measuring one inch in diameter. A rounded piece of cork or india rubber, one inch in thickness, was fitted accurately into the apex of the cone, and a long thin rod reaching down to and resting on the rubber or cork was then inserted and held in the middle of the cone while the shot was poured around it, until the bag weighed about twelve pounds. A piece of canvas, with a hole in the center, for the passage of the rod, was then stitched over the base of the bag, and a stout wire hook fastened to its center. The bag was suspended to a pulley in the ceiling by means of a rope, with which it was connected by a piece of rubber tubing and a large linked chain. The tubing made the apparatus elastic, and the chain enabled the Doctor to regulate more easily the amount of pressure employed.—*N. Y. Medical Journal.*

INTESTINAL OBSTRUCTION TREATED BY ELECTRICITY.

M. GIOMMI (*Centralblatt*) gives an instance of the benefits of electricity in intestinal obstruction in the case of a man, aged fifty-one, who, up to the time of his illness, had always been strong and well. He had had complete obstruction for some weeks, and was almost moribund when the idea struck M. Giommi that the cause of the evil lay in atony of the walls of the bowel. He accordingly applied a strong induced current several times during two days, each time for ten or fifteen minutes. One electrode was introduced into the rectum and the other applied to the abdominal wall, in the course of the transverse colon. The result was marvelous; copious stools followed the free escape of gas, and after a few days the patient left, quite well.

CURE OF INTUSSUSCEPTION BY SODA WATER.

AT a late clinic at St. Bartholomew's Hospital, London. Dr. Southey narrated the case of a young woman who had been attacked three days before, on board ship, with great abdominal pain, complete obstruction of the bowels, and vomiting, becoming fecal. She had been treated by the ship's captain with ten grains of calomel and one scruple of jalap powder every two hours. This occurred during a menstrual period, and suggested possible obstruction from pelvic hæmatocele; examination, however, did not confirm this idea. She was evidently very ill. The rectum was emptied by enemata; no obstruction could be felt by the finger; the loins were hollow and resonant, and the hypogastrium hard, indicating an empty colon and loaded small intestines. The long tube was then passed, but was soon arrested by an obstruction (very probably the promontory of the sacrum); only a little fluid could be injected, and that caused great pain. It was proposed to the patient to administer chloroform before proceeding further, but she declined it. The question of abdominal section was proposed, but it was decided to try the following plan first. The patient was placed in the genu-pectoral position, the tube again inserted, and the anus held closely around it. A syphon bottle of soda water was then connected with the tube, the bottle being placed in hot water to increase the elasticity of the gas contained in it. On opening the valve of the bottle and allowing the gas and water to ascend into the rectum, enormous abdominal distention and severe pain resulted. Two syphons of soda water were thus used. The patient then felt "something give way or burst," and copious action of the bowels followed. Probably some band of adhesion gave way, or a twisted portion of intestine was unfolded. In two days the patient was well enough to undertake a journey. No medicine was used, except subcutaneous injections of morphia. The plan was recommended two years ago in one of the French reviews, and it seemed preferable to the simple injection of fluid the elastic gas forcing its way more easily than fluid would do.

CURE BY THE USE OF SPECTACLES.

IN a recent lecture Dr. J. Hughlings Jackson, Physician to the London Hospital, says:

In some cases of abnormal refraction there is a simulation of brain disease, or, at any rate, patients who only want appropriate glasses are occasionally treated by physicians for brain disease. I have been speaking of hypermetropia, but the remark applies to other abnormalities of refraction.

Mr. Brudenell Carter has reported a remarkable case of myopia simulating brain disease, in the eighth volume of the Clinical Society's transactions. This report should be carefully studied by physicians. The patient, for supposed brain trouble, took a voyage to Australia, but was no better for it. He was told that "he must abandon the idea of carrying on the family business, or of taking any active part in life."

This patient was immediately thoroughly and permanently cured by the adaptation of appropriate glasses. The possibility of an anomaly of refraction or any eye defect being at the bottom of that patient's trouble seems not to have occurred either to himself or to his doctors. He did not consult Mr. Carter for any defect of sight, but because he had heard that ophthalmic surgeons had an instrument useful in the investigation of disease of the brain. I could relate several cases of hypermetropia in which the diagnosis of brain disease had been made by physicians and refuted by ophthalmic surgeons. Here is one:

A medical student, twenty-one years of age, had been obliged to give up his work because reading brought on attacks of vomiting and frontal headache. The vision of each eye, for near and distant objects, seemed good to ordinary examination, and there were no ophthalmoscopic changes, except some dilatation of retinal veins. To cut a long story short, after traveling about two years, doing nothing toward his cure, Mr. Tweedy, in May last, fitted him with a pair of spectacles, remedying hypermetropia and astigmatism. The patient has been well ever since; has returned to his medical career, and has graduated at the University of London. Without some knowledge of anomalies of refraction, the physician would never suspect the real nature of such a case as that of this medical student.

Mr. Carter writes, in his paper referred to, that his advice was received by the patient and his father with polite incredulity, although fortunately it was acted on. I find that patients whose symptoms appear to them to depend entirely on something wrong in the head or in the liver receive with something more than incredulity the statement that they require glasses, and I fail now and then to make them consult an ophthalmic surgeon. Yet it is very ludicrous to give "nervous" and other remedies for a state of things which requires only a scientifically adapted mechanical aid.

TREATMENT OF HYDROPHOBIA BY OXYGEN.

A GIRL, seven years of age, was bitten by a rabid dog. The wound, which involved the subcutaneous cellular tissue, was at once cauterized with nitrate of silver and healed completely in seven days. The child had suffered three months previously from diphtheria, which had left a paralytic aphonia. When the wound had healed the child became very excitable. Seventeen days later dyspnœa suddenly manifested itself. The inspirations were free but exspiration was difficult and interrupted. Deglutition was almost impossible; the pulse was rapid and the fingers contracted. Neither urine nor fæces were passed for forty-eight hours. The child inhaled three cubic feet of oxygen, which relieved the symptoms in two hours and a half. The next day a more severe attack occurred, with spasms of the muscles of the back and loins, spasmodic respiration, and complete insensibility. These symptoms were again removed in three-quarters of an hour by the inhalation of oxygen. The slight dyspnœa which remained was treated in the same manner with oxygen for ten days, and the child made a complete recovery, with the aid of the monobromate of camphor, which was continued for two weeks.—*Wratschebuija Wedomosty,* No. 36.

CURE FOR RATTLESNAKE BITE.

By J. J. KNOTT, M.D., of Atlanta, Ga.

ON the evening of July 5, 1876, a man by the name of Burney was brought to me for treatment, having received a bite from a large size rattlesnake some fifteen minutes before I saw him. The wound from the bite was over the metacarpo-phalangeal joint of the index finger of the left hand. Previous to my seeing him, he had administered to him, at a neighboring drug store, a large drink of whiskey. This was about five minutes before I saw him.

He was in the following condition when brought to me: The body bathed in a profuse clammy perspiration; no pulse at wrist; respiration abdominal and irregular; pupils completely dilated; inability to swallow. I immediately injected a half drachm hypodermic syringeful of a solution of carbonate of ammonia into one of the superficial veins of the left hand. The solution was of the following strength:

℞. Carb. ammoniæ, gr. xl.
Aqua destillata, ℥ j. M.

This injection was followed immediately by an almost imperceptible pulse at the wrist. Waiting awhile to see the full effects of this injection, I found it was only temporary. The patient had fallen back into a worse condition than before the injection. I now exposed the arm, by removing the coat and shirt sleeves, and with many misgivings as to the result proceeded to inject into the cephalic vein a syringeful of the above solution.

There was an immediate response to this in the radial artery, producing a pulse of about thirty to the minute, and of good volume; the second injection into the cephalic vein was followed by still further improvement in the patient's condition. On making the third injection, the patient sat up, and could distinguish objects; on the fourth injection arose to his feet, apparently as well as before the reception of the bite. After remaining a few moments he left, in company with some friends.

I endeavored during the night to find out his whereabouts, but was unsuccessful in learning anything of him until ten o'clock the next morning, when I found him on the second floor of a building on Marietta street, in a room used as a paint shop. When found, he was lying prostrate on the floor in a perfect stupor. My first impression was that, perhaps, he had been heavily dosed with whiskey during the night, though on examination of his pulse and the pupils of his eyes I was at once satisfied that such was not the case; the pupils were fully dilated; pulse feeble and irregular. I found on examination the hand very much swollen, also the left forearm, halfway up to the elbow.

Drs. G. G. Ray, King, Wylly, and A. R. Alby saw the case with me, and we proceeded to make the injections of ammonia; some twenty-five injections were made in the course of an hour, under which the patient gradually returned to consciousness and sight; circulation resumed its natural frequency and volume. I now determined to make a comparative test of the whiskey treatment and carbonate of ammonia injections. I accordingly ordered whiskey in large doses every fifteen minutes. This was continued until fifteen minutes after two o'clock P.M., when I saw the patient again.

He had now consumed one and a half pints of whiskey in the course of an hour and three quarters. He was still bright, but showed no indications of being under the influence of the whiskey. Continue whisky. Four o'clock P.M., saw the patient with Drs. Ray, Alby, and Wylly: patient had fallen back into stupor, with feeble pulse, dilated pupils, and abdominal respiration; commenced the injections of ammonia, which were continued at regular intervals until six o'clock P.M., when, the patient being in a condition to be removed to more comfortable quarters, I procured a cab and had him removed to his house, about a mile distant, and after he reached his home I continued the injection at regular intervals, until ten o'clock P.M. At nine o'clock I administered sulph. quinine, 8 grains, with a view of overcoming any depressing effects of the poison on the great sympathetic nerve.

I now left the patient with the following instructions for the night:

Carbonate of ammonia every hour in whiskey and water; 8 grains of sulph. quinine, to be given at twelve o'clock. Should the patient be disposed to sleep, the sleep appearing natural and pulse keeping up, to be allowed to do so for an hour, without being disturbed.

July 7th, six o'clock A.M., was called to see the patient, the messenger informing me that he had gone back into a stupor. On arrival, I learned the following particulars of his condition through the night:

Between twelve o'clock and three A.M., voided urine very freely, being the first that he had passed since the reception of the bite. At three o'clock the patient dropped off into a quiet sleep, and was not disturbed until half-past five o'clock, at which time he became restless; on attempting to arouse him, the attendants found that he was becoming stupid, with loss of sight. I found the patient in a stupor, but not so profound as before; the sight also gone, with some irregularity about the pulse.

I immediately proceeded to inject the ammonia, which was followed by an immediate subsidence of all unfavorable symptoms; only four syringefuls were injected. I now ordered sulph. quinine, two and a half grains every two hours, carbonate ammonia and whiskey every hour, as before; directed the quinine to be given half an hour from the time of administering ammonia, to prevent, as far as possible, the formation of free quinia.

Twelve o'clock, M.—No return of unfavorable symptoms. At this visit the patient's bowels moved very freely; treatment as before continued; in addition, to have hydrarg. sub. mur., gr. j., sach. alba, grs. iv., every three hours, until action from the bowels indicated its effect on the liver. Nourishment, beef tea.

Six o'clock, P.M.—No return of symptoms; on examination, found that phlebitis had set in; the veins into which the numerous injections had been made presented a knotty appearance, with the peculiar purple discoloration along their track. Ordered cold applications to the parts, the cloths to be reapplied every ten minutes; quinine discontinued until morning; carbonate ammonia and whiskey every four hours.

July 8th.—Patient slept very comfortably through the night; calomel acted; hardness had subsided in veins, but slight discoloration remaining; cold applications continued; carbonate ammonia and whiskey three times a day; quinine two grains, three times a day. Patient now able to be up and about.

Six o'clock P.M.—Patient dismissed. Cured.

Remarks.—To Professor Halford, of Australia, belongs the credit of the introduction of this, as it appears to us, magical treatment in the bites of venomous reptiles. In the case just cited, the poison was felt (so the patient informed me, after his recovery) not exceeding two minutes from the reception of the bite; the first symptoms being nausea and giddiness, with loss of sight.

One point I noticed particularly: on making an injection while the patient was fully under the influence of the poison, the blood that escaped from the vein, by the needle of the syringe, was of an inky appearance. On the second injection, though made on the opposite and immediately following the first, the blood that escaped presented more the appearance of arterial than venous, which shows very plainly the powerful effect exerted by the ammonia in changing the condition of the blood.

From what I observed in this case, I am satisfied that there is a broad field open for still further investigation in the application of this treatment to other affections than the poisonous effect of the bites of venomous reptiles and insects. How many of us have often observed the happy effects following the administration of ammonia in the numerous adynamic diseases, when administered by the stomach!

What may we not expect from the immediate application of this remedy to the blood, instead of sending it through a chemical laboratory, thereby entering the circulation in the form of we know not what! From my experience in this case, I am satisfied that we have but little to fear from the introduction of air into the veins while employing ammonia, as I believe that the ammonia prevents any serious effects from the introduction of air. Owing to the apparently hopeless condition of the patient, I paid but little attention to this point, and I have no doubt but that air was introduced during some of the injections, yet I noticed no bad effects from it.

The above case, I believe, is the first in which this treatment has been employed in this country; at least, I have seen no reports of any cases previous to this. I am thoroughly convinced, from my observations in the above case, that when this treatment is employed and persevered in, it will

prove as near a specific as anything we employ belonging to the so-called specifics.—*Med. and Surg. Reporter.*

WE learn from our Chicago exchanges, says the *American Architect*, that the pollution of the lake water by the city drainage has so far extended that the water supply, which, as is well known, is drawn from Lake Michigan through a tunnel carried far out to deep water, has become seriously affected. Prof. Piper draws attention to the discovery in water, taken from the hydrants, of foreign and deleterious maters, among others of certain spores or eggs which he considers dangerous to the health of the community, in the same manner as the spores of the *bothriocephalus*, a certain worm which formerly was the cause of serious diseases among the inhabitants of the shores of the Lake of Geneva, but which entirely disappeared when those waters were purified by the application of various sanitary appliances to the drainage of the bordering towns, notably by changes in the construction of water-closets, which, up to that time, had emptied themselves into the lake. At present the refuse of the towns is carefully collected for the purpose of manuring the land, and many of the former local forms of disease are now unknown. In like manner, as Prof. Tyndall has observed, in the district of Novgorod, in Russia, between the years 1867 and 1870, 828 human beings and 56,000 horses, cattle, and sheep perished by the drinking of water containing the spores of the *bacillus antracis*. But lest those who are not habitual water-drinkers may derive some comfort from these unsavory statements, Prof. Piper assures his fellow-citizens that the same water in the form of beer or mixed with whiskey is even more deleterious, and that those who indulge in such beverages are the first to succumb to the forms of disease which are generated by these causes.

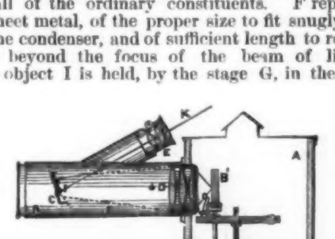
NEW REFLECTING MAGIC LANTERN.

By J. B. KNIGHT.

THE projection on the screen of the images of small opaque objects is often a very desirable means of illustrating lectures or papers read before societies, but has not been very largely employed, because of some difficulties attending its use.

As the light for forming the image is only that emanating or reflecting from the surface of the subject, it is important that it be strongly illuminated. The arrangement about to be described was devised to secure a better illumination than is obtained with the ordinary megascope, and at the same time save the cost of a special instrument, by utilizing the ordinary projecting lantern with which nearly all our educational institutions are provided.

The arrangement for accomplishing this purpose is shown in the accompanying cut, in which A represents the lantern box, B the lime, C the oxyhydrogen gas jet, and D, the condenser—all of the ordinary constituents. F represents a tube of sheet metal, of the proper size to fit snugly over the ring of the condenser, and of sufficient length to reach considerably beyond the focus of the beam of light from it. The object I is held, by the stage G, in the beam of



light, as near its focus as possible, and yet give the proper breadth of illumination. This stage G is supported by and hinged across its center to the inner movable tube R. To the tube F is attached a branch for holding the focussing lens at the proper distance from the illuminated side of the object, with its axis at as small an angle from the axis of the condenser as will permit the cone of light passing from it to the screen to clear the top or sides of the lantern box. Objects are easily placed on the stage G by withdrawing the inner tube R, and are easily adjusted by this longitudinal movement and the angular movement of the stage on its hinges.

This attachment is simple and inexpensive, and by it the usefulness of the projecting lantern is greatly increased.—*Journal of the Franklin Institute.*

CAN WE TRANSMIT POWER IN LARGE AMOUNT BY ELECTRICITY?*

By N. S. KEITH, Newark, N. J.

THIS question is suggested by a statement made by Dr. Siemens, widely printed in the journals of the day, that a continuous rod of copper thirty miles in length and three inches in diameter is capable of conveying that distance, electrically, energy equal to 1,000 horse power. It is not attempted to advance the statement that the source of power shall be zinc, nor even coal, but waterfalls, which from their situation are not practically available for manufacturing establishments in their immediate vicinity.

In order to fully consider this subject, we must understand the doctrine, I may say the science, of the correlation of the forces, so called. We must understand that all matter is endowed with an amount of FORCE, and that each atom and molecule, simple and compound, has its specific portion of the whole. This force, at rest, is called latent heat, intrinsic energy, or potential. In motion it is called heat, light, electricity, chemical affinity, attraction, magnetism, power, etc., according to its sensible manifestations. These are the effects of the one force in motion in different substances, or in different assemblages of matter. Force put in motion comes to rest by reason of the resistance to motion which it encounters; in overcoming of resistance the manifestation is sensible heat. Each of these manifestations of force is convertible to one or all of the others, and they are all caused by some mode of motion. Force may be illustrated by a spring under tension, or by a suspended weight. Release the spring and weight, and they give off as much force or energy as was used in setting up the tension of the spring, and in raising the weight perpendicularly the length of its fall. Any of these forces then may, figuratively speaking, release the weight and spring.

While there is but one electricity, there are two conditions of it, namely, *static*, which is electricity at rest but under high tension, and *voltaic* electricity or *galvanism*, which is a mode of motion. It has but one cause, and that is force, or matter in motion. Yet we, for perspicuity, call electricity by friction, *static* or *frictional*; electricity by chemical affinity, *chemical* from its immediate cause, or *voltaic* or *galvanic* from its discoverers; electricity by magnetism, *magnetic*; electricity by heat, *thermic*; electricity by mechanical power, *dynamic*.

As chemical and thermic electricity have too costly sources for our purpose, we must consider the magnetic and dynamic.

Now that we have learned what electricity is, we must understand what it is not. As an entity it does not exist; it is a signification simply. When we comprehend it as a condition or quality of matter in self-containing motion, not as a current or flow of something through matter, we will be able to deduce facts in the science, and sustain them by practical illustration. The first conception by the mind of a force or motion having its source within a circuit, and manifesting itself at all parts thereof, is that of a current or flow of something. The probability is, electric current is molecular change of form caused by tension upon the atoms composing the molecules in the direction of disrupting them. There is certainly a change of dimensions of matter subjected to electricity, as there is with heat and magnetism. This change of form causes friction of adjacent molecules and its resultant heat. This heat is the exact equivalent of the energy causing the electric current. Energy, when used as electricity, is called *electromotive force;* this varies in degree with its tension, as in case of its illustration by a weight in suspension or by a spring. Some use the term *intensity* to express the same. The tension of a spring may illustrate the electromotive force of static electricity, which imparts its charged energy with a single impulse. A suspended weight released increases its speed each foot of fall, and consequently its force and effective quantity. So with voltaic electricity: each cell in circuit increases the speed and quantity of current. In case of dynamic electricity, each increment of circuit receiving electric impulse adds to speed and quantity of current.

All matter offers an amount of resistance to changes of form or arrangement of molecules, whether by heat, electricity, magnetism, or any other of the forces. This resistance is specific for each substance. The specific resistances opposed to electromotive force have been tabulated relatively in the cases of metals common in the arts, and with the important alloys. The metals are the best conductors, or, in other words, offer the least resistance. These follow solutions of binary salts, other liquids, *et sequentes*. Copper and silver offer the least resistance, are relatively alike, and are graded as 1 in scale of resistances. Iron offers nearly six times the resistance of copper, and is graded 5·95. Heat increases the resistance of metals to the extent of about 0·2 of 1 per cent. for each degree Fahrenheit rise in temperature.

Electricians have a formula which sets forth Ohm's law. This is that the current (sometimes called quantity) of electricity is the result obtained by dividing electromotive force by resistance; thus, $\frac{E}{R} = C$.

The unit of electromotive force is called a *volt*, in commemoration of Volta, the inventor of the voltaic pile. It is very nearly represented by the electromotive force, or energy, or intensity of a Daniell cell. The unit of resistance is called an *ohm*, after Ohm, who laid down the law. A wire of pure copper 6,046·5 feet in length and ¼ of an inch in diameter has a resistance of one ohm. The unit of current or quality is called a *weber*, or *veber*, after Weber, another investigator in the line.

A veber of current represents the energy set free by the combustion of 11 grains of carbon, or 11 grains, about, of coal, or 1 grain of hydrogen, with a development of 6 units of heat in 6,338 seconds. That amount of free or sensible heat is set free in the circuit. Thus, one volt of electromotive force forces one veber of electric current through a circuit of one ohm resistance, requiring to do so 4,673 foot pounds of energy, with a development of 6 units of heat in the circuit in 6,338 seconds. The heat set free is the exact measure of the force used.

If we pass this veber of current through a solution of copper sulphate, the electric equivalent amount of metallic copper will be deposited, namely, 31·75 grains in the same time. Now, if we increase electromotive force by adding another cell *in* the circuit, making electromotive force 2, and so regulate resistance that it remains one ohm, a current of 2 vebers passes in the same time, thus: $\frac{2\ E}{1\ R} = 2\ C$.

Now, we find that twice as much zinc is consumed in *each* cell, or four times as much in the circuit, or its equivalent in energy is used in depositing only twice as much copper. We have in the circuit four times as much heat, which is the measure of the energy expended. Chemical decomposition is the measure of current, while heat is the measure of electromotive force multiplied by current. Increase electromotive force to 3, keep resistance 1, and we have a current of 3, and nine times the energy expended, resulting in nine times the heat.

It is now to be seen that by increasing definitely the amount of electromotive force, and at the same time keeping resistance as low as possible, we may use a definite amount of energy and distribute it as heat throughout the circuit in proportion to the special resistance of its parts, and utilize it as mechanical power. The object of increasing E at the expense of C is that we may save in weight of copper constituting the conductors. We get the energy distributed throughout the circuit, though but the square root of it is shown in chemical action when measured by the amount of copper or other metal deposited in a single depositing cell.

If we magnetize a core of soft, uncarbonized iron within a coil of copper wire by bringing it into the magnetic field of an electro or a permanent magnet, at the instant of stoppage of motion a current of electricity will start in the coil in one direction; that is, the molecules composing the circuit will turn in one direction, and then the action ceases. Remove the core and coil, a reversed current starts and continues as long as motion lasts in removing them from the field. If we revolve this arrangement between the poles of a magnet, thus alternately magnetizing and demagnetizing the core, we will get a succession of discharges of magnetism through the copper coil utilized as electricity. While the core is acquiring magnetism, there is no current in the coil, as there is no magnetic resistance to motion which requires force to overcome. As soon as it begins to lose magnetism an electric current is induced in the cell, which we may cause to do work by proper mechanical appliances.

We will find that the coil and core are heated, and the amount of heat is the measure of the mechanical force used, less that due to friction of the journals carrying the arrangement. If the coil completes the electric circuit within itself, so that there is no external resistance, then the total heat will be developed therein. If the circuit is made complete by a conductor, then the heat will be divided between the coil and conductor in proportion to their respective resistances. If this conductor be the coils of an electromotor, the heat due to it can be utilized as work, less loss by conversion.

We have now the general requirement laid down, so will proceed to plan and construct a theoretical machine to suit the requirements of 1,000 horse-power, to be transmitted, if possible, through a rod of copper thirty miles in length and three inches in diameter. As resistance of wire of same diameter is in direct proportion to its length, and as we have seen that 6,046·5 feet of copper wire, ¼ inch in diameter, has a resistance of 1 ohm, so 30 miles, or 158,400 feet of ¼ inch wire, has 26 ohms resistance. But as it also decreases in proportion to the square of the diameters, we figure in the 3 inch rod a resistance of ·18 ohm, if of pure copper, at a temperature of 60° Fahr.

The energy of 1,000 horse-power is measured as 33,000,000 foot pounds per minute, and that of one veber current equals 4,673 foot pounds in 6,838 seconds, or 44·24 foot pounds per minute. So it will require 746,000 vebers current, or their equivalent in energy, to utilize 1,000 horse-power as electricity for dynamic purposes.

We may therefore use electromotive force of 1,000 volts, resistance of 1·34 ohms, and a current of 746 vebers; thus, $\frac{1{,}000\ E}{1{\cdot}34\ R} = 746\ C$. In other words, the dynamic equivalent of 746,000 vebers may be had by multiplying the electromotive force 1,000 by the current 746.

It has been found that a discharge of the magnetism of a soft iron core induces a current in the coil surrounding it possessing electromotive force of one volt for about each twenty-five feet of coil. The quantity or current comes from the strength of the magnetism and number of discharges. For 1,000 volts electromotive force we will take 25,000 feet in length of copper wire or strips, weighing 1·2 pounds per foot length, or in all 30,000 pounds. This will have a resistance of ·66 ohm. It should be wound upon a core of iron weighing 10,000 pounds. This core and coil, constituting what is called an armature, must be revolved between the poles of an electric magnet having such an attraction for the armature as to call for the expenditure of 1,000 horse-power in revolving it. Such a magnet will weigh, probably, 60,000 pounds, and have a like weight of copper in its coils. It should be excited or magnetized by a smaller armature revolved between the poles of a smaller magnet, with an expenditure of, say, 100 horse power. This is necessary, because, if the coil of the magnet is part of the main circuit, the resistance will be much increased.

The electro-motor receiving the current of electricity must have at least the same length of copper in its coils; and as the resistance of the coils (when the machine in motion is exerting its greatest power) is double that which they have at rest, and as it is necessary from our other fixed resistances to make the resistance of the machine ·50 ohm, we make the weight of copper coils per foot 3·17 pounds, a total of 79,200 pounds, with a weight of iron about 70,000 pounds.

The cost of this apparatus will be as follows:

Exciting magnet and armature...............	$3,000
Large magnet, 60,000 lb. iron, including work thereon, 10c. per lb............	6,000
60,000 lb. copper, 30c. per lb....	18,000
Armature: 10,000 lb. iron, and work thereon, 10c. per lb............................	1,000
3,000 lb. copper, 30c. per lb.	9,000
Brass bearings, brushes, etc..........................	2,000
Total for machine..................	$40,000
Conductor: 158,400 feet copper rod at 27½ lb. per foot, 4,356,000 lb., 30c...........	$1,306,800
Ground plates and connections......	5,000
Insulation, etc., indefinite, say	100,000
Total for conductor.........	$1,411,800
Motor: Iron and work, 70,000 lb., 10c...........	$7,0[illegible]
Copper, 79,000 lb., 30c..................	23,7[illegible]
Brass, brushes, etc.......................	2,[illegible]
Total for motor	[illegible]

The energy of 1,000 horse-power expended on the machine generating the electric current is distributed throughout the circuit in proportion to the special resistances of several parts. The armature, having a resistance of ·66 ohm, absorbs 66-134 or 492·5 horse-power; the conductor 18-134 or 134·3 horse-power; the motor 50-134 or 373·2 horse-power. This last amount is all that can be utilized with this arrangement, even if there is no loss. We may make our electric machine and the motor larger, or place two of them, making the resistances of them one half as much, but not with any increase of utilizable power, as the resistance of the conductor remains the same.

Let us consider a resistance of ·33 ohm for machine, ·18 ohm for conductor, and ·25 ohm for motor, and we have 33-76 or 434 horse-power for machine, 18.76 or 237 horse-power for conductor, and 25-76 or 329 horse-power for motor. This is less available power than before. The resistance of the earth returning the current we may count as nothing. Under no circumstances can we utilize the full power expended.

If we decrease the resistance of the machine to ·33 ohm, and increase that of the motor to ·83, keeping total resistance the same, we will gain. Then the machine will absorb 33-134 or 246·2 horse power; the conductor 18-134 or 143·3 horse-power; and the motor 619·5 horse-power. With a larger conductor or shorter distance, this proportion may be increased.

There are various sources of loss, especially with electricity of such electromotive force and tension. I have no doubt that at least 50 per cent. of the energy expended on a magneto-electric or dynamo-electric machine at a waterfall may be used at a distance by an electro-magnetic motor as mechanical power.

The amount of heat developed throughout the entire circuit will be equivalent to that from the combustion of 2[illegible] lb. of coal per hour, or 42·746 heat units per minute. The proportion due to the armature, having resistance of ·33 ohm, is sufficient to raise its temperature one degree Centigrade per minute. Of course, then, some arrangement for cooling by water must be applied.

What the effect of a discharge of a portion even of this current, with its high tension, through the body of a man would be, I leave you to imagine. Probably it would be worse for that man than it was for the one who was kicked by the Kentucky mule; "they swept up the pieces" in that case, as the veracious reporter states.

* A paper read before the American Institute of Mining Engineers, at the Wilkes-Barre meeting, May, 1877.

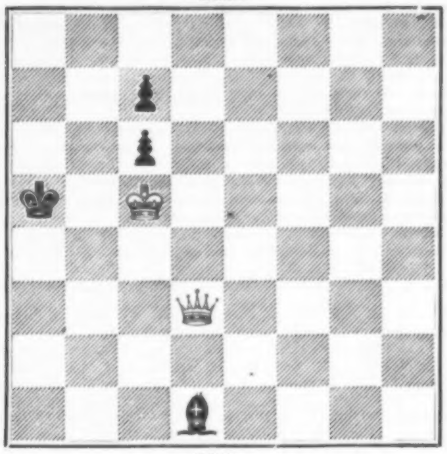
Scientific American Chess Record.

[All contributions intended for this department, may be addressed to Samuel Loyd, Elizabeth, N. J.]

Problem No. 7.—Centennial Tournay.

Motto: "God save the Queen." By Samuel Loyd.

Black.

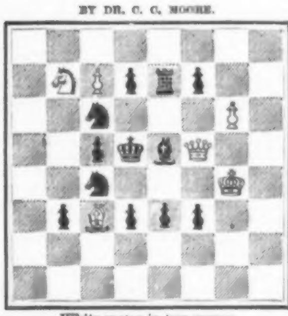


White.

White to play and mate in three moves.

MRS. J. W. GILBERT.

BY DR. C. C. MOORE.



White mates in two moves.

ELIEVING that a likeness of Mrs. J. W. Gilbert, of Hartford, Ct., would possess great interest to all lovers of chess in America, we take especial pleasure in gracing our gallery of this week with her portrait.

Wishing to avoid a plurality, or, as in the case of Morphy, a confusion of likenesses with but little resemblance in common, we acknowledge an international compliment, and reproduce from the *Westminster Papers* a picture which we are assured is a faithful likeness of the acknowledged Queen of Chess. Mrs. Gilbert is generally admitted to be the most accomplished lady chess player living, and as a successful player of games by correspondence has achieved a world-wide reputation. The specimens of her play which we give this week surpass anything recorded from actual play, for brilliancy of problematical termination, that has yet come under our notice.

CHESS JOURNAL "B" TOURNAMENT OF '76.

Our initial letter of this week was the winning problem of the Letter Tournament of last year, wherein two prizes were offered for the best problem in the form of the letter "B." The first prize was won by Dr. C. C. Moore, the second prize by Master Harry Boardman, whose problem appeared in our last week's issue. Dr. Moore offered the amount of his prize to inaugurate a second Letter Tournament, which, as has been already mentioned, resulted favorably to *our* editor.

THE STATES vs. CANADA.

An international correspondence tournay was inaugurated during the early part of last year, between twenty-nine American and the same number of Canadian players. The result has been a decided victory by a score of more than two to one for the Americans.

Mrs. J. W. Gilbert took part in the contest, and we are pleased to present her games to our readers as mementoes of this interesting tournament, as well as specimens of her remarkable talent.

(QUEEN'S KNIGHT'S GAMBIT.)

Mr. A. Hood, of Wroxeter, Ont.	Mrs. J. W. Gilbert, of Hartford.
1. P to K 4	1. P to K 4
2. Q Kt to B 3	2. Q Kt to B 3
3. P to K B 4	3. P x P
4. P to K R 4	4. B to K 2
5. Kt to K B 3	5. P to Q 3
6. P to Q 4	6. B to Kt 5
7. B to Kt 5	7. B x R P ch
8. K to B sq	8. B to B 3
9. P to Q 5	9. P to Q R 3
10. B to R 4	10. B x Q Kt
11. P x Q Kt	11. P to Q Kt 4
12. P x B	12. P x B
13. Q to Q 4	13. P to K B 3
14. B x P	14. Kt to K 2
15. Q x R P	15. Castles
16. Kt to Q 4	16. Kt to Kt 3
17. P to Kt 3	17. Kt to K 4
18. K to B 2	18. Q to K sq
19. Q R to Q Kt sq	19. P to Kt 4
20. B to Q 2	20. P to K B 4
21. B x Kt P	21. P x P dis. ch
22. K to K 3	22. P to Q 4
23. B to B 4	23. Kt to B 5 ch
24. K to B 2	24. P to K 6
25. K to Kt sq	25. P to K 7

26. R to K sq, and Mrs. Gilbert announced mate in twelve moves!

(RUY LOPEZ.)

BETWEEN THE SAME PLAYERS.

Mrs. Gilbert.	Mr. Hood.
1. P to K 4	1. P to K 4
2. Kt to K B 3	2. Kt to Q B 3
3. B to Kt 5	3. P to Q R 3
4. B to R 4	4. P to Q Kt 4
5. B to Kt 3	5. B to Q B 4
6. P to Q B 3	6. P to Q 3
7. P to Q 4	7. P x P
8. P x P	8. B to Kt 3
9. B to Q 5	9. K Kt to K 2
10. Kt to Q B 3	10. Kt x B
11. Kt x Kt	11. B to R 2
12. Q to B 2	12. B to Kt 2
13. B to K 3	13. Castles
14. Castles K R	14. R to B sq
15. Q R to Q sq	15. Kt to K 2
16. Kt to K B 4	16. Q to Q 2
17. Kt to K Kt 5	17. P to K R 3
18. K Kt to R 3	18. P to Kt 4
19. Kt to R 5	19. P to K B 4
20. Q to Kt 3 ch	20. K to R 2
21. B x Kt P	21. P x B
22. Kt x P ch	22. K to R 3
23. Q to K R 3	23. K x Kt
24. R to Q 3	24. K to Kt 3
25. Kt to B 4 ch	25. K to B 2.
26. Q to R 5 ch	26. Kt to Kt 3
27. Kt x Kt	27. K to K 3
28. P to Q 5 ch	28. K to B 3
29. R to K B 3	29. K to Kt 2
30. Kt x R	30. R x Kt
31. Q to Kt 5 ch	31. K to B 2
32. R x P ch	32. Q x R

33. P x Q, and mates in eleven moves!

MRS. J. W. GILBERT.

(RUY LOPEZ.)

The following game, with a still more bewildering termination, was recently played by correspondence between Mrs. Gilbert and Mr. W. S. Berry, of Beverly, Mass. The notes are by Mrs. Gilbert.

Mrs. Gilbert.	Mr. Berry.
1. P to K 4	1. P to K 4
2. Kt to K B 3	2. Kt to Q B 3
3. B to Kt 5	3. P to Q R 3
4. B to R 4	4. Kt to B 3
5. Castles	5. Kt x P
6. R to K sq	6. Kt to B 4
7. B x Kt	7. Q P x B
8. P to Q 4	8. Kt to K 3
9. P x P (*a*)	9. Q to K 2 (*b*)
10. Kt to B 3	10. B to Q 2
11. P to Q R 4	11. Castles Q R
12. P to Q Kt 3	12. P to K B 3
13. Q to K 2 (*c*)	13. Q to B 2
14. Kt to K 4	14. R to Kt sq
15. P to Q B 3	15. P to K R 3
16. P to Q Kt 4	16. P to K B 4
17. Kt to Kt 3	17. P to K Kt 4
18. Kt to Q 4	18. Kt x Kt
19. P x Kt	19. R to K sq (*d*)
20. P to Kt 5	20. B P x P
21. P x P	21. B x P
22. P to K 6	22. Q to Kt 3
23. Q x B	23. P to B 5, and Mrs.

Gilbert announced mate in nineteen moves!!

(*a*) This is not the book move, but it seems equally as good.

(*b*) We prefer B to Q 2.

(*c*) P x P would divide black's pawns, but it would also prove damaging to white by opening a file for a strong attack on the right wing.

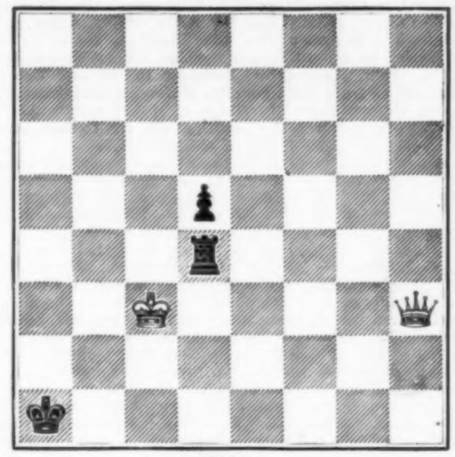
(*d*) Better than taking P with B; for if the latter, black would lose a piece.

By reference to our notice of the grand international correspondence match between England and the United States, it will be seen that Mrs. Gilbert has taken a hand and seems to be pitted against Mr. Gossip, if our arrangement of the players is correct. No game of the tournament will be looked forward to with more interest than this one, and we feel sure that our fair champion will gain new laurels.

Problem No. 8.—Centennial Tournay.

Motto: "God save the Queen." By Samuel Loyd.

Black.



White.

White to play and mate in four moves.

SOLUTIONS TO PROBLEMS.

No. 1.—By Dr. C. C. Moore.

WHITE.	BLACK.
1. Kt to Kt 4	1. K to R 5
2. Q to Q B 4	2. K moves
3. Mates.	
	1. If P to Kt 3
2. Q to R 6	2. K moves
3. Q to R 2, mate	
	1. P to Kt 4
2. B to K sq	2. K moves
3. Q to R 2, mate	
	1. K elsewhere
2. Q to B 4	2. Any move
3. Mates	

No. 2.—By Dr. C. C. Moore.

1. Kt to R 2	1. K to R 7 x Kt
2. Kt to B 2	2. K to Kt 6
3. B to Kt 5	3. K to R 7
4. B to B 4, mate.	
	1. If K to R 6 x Kt
2. K to B 2	2. K x Kt
3. B to Q Kt 2	3. P to K 4
4. B to Q 5, mate.	

OUR SOLVERS' TOURNAY.

We will be pleased to receive correct solutions from our correspondents, accompanied by the exact time required to solve each problem. Quarterly prizes of the Scientific Supplement for one year will be awarded every three months for the best score of all the problems published. This tournament commences with Problem No. 1, the only stipulation being that solutions must be received before their publication.

BLACKBURNE vs. STEINITZ.

As we surmised, the match between Messrs. Blackburne and Zuckertort has been declared "off." Mr. Blackburne, however, seems determined that the public shall not be disappointed, and has boldly challenged Steinitz to a return match for $500, which *defi* has been promptly accepted. Whether Blackburne retrieves his laurels, or Steinitz repeats his overwhelming victory which we recorded a few weeks since as the result of the first encounter, our artist, who has been on the "spot," promises to have a portrait of the victor ready.

SCIENTIFIC QUEEN PROBLEMS.

We select two of our centennial problems, which were entered through the Detroit *Free Press*, under the motto of "God save the Queen," and as a still further contribution appropriate to the occasion, we would be pleased if any of our solvers can furnish solutions to the following queen problems.

I. Place the queen on the board, and in fifteen moves pass her over every square of the board, returning to the starting point.

II. In placing eight queens on the board so that no one queen is attacked by another, which square must in every case be occupied by one of the queens?

III. How many ways can five queens be placed on the board so as to guard every square of the chess board?

HISTORICAL CHESSMEN.

Mr. F. Lemon, of this city, has designed a very beautiful set of chessmen, which is rather instructive in a historical point of view. The set consists of a series of statuettes representing historical personages of the time of Richard 1st, of England, Cœur de Lion. On one side the set embraces statuettes of Richard and his lovely Queen, Berengaria, with celebrated archbishops, knights, castles, and fighting men for pawns. On the other side we have Philip, of France, with his queen, archbishops, knights, fighting men, etc. All the principal figures represent notable characters and are costumed with historical accuracy and careful attention to details in the representation of appropriate insignia and ornamentation. The figures of the knights in fighting attitude, of the pawns with shields and spears, in battle array, make a very interesting appearance upon the chess board. On some future occasion we may be able to give further particulars.